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*be returned to E. Hare
1 Leinster Place West
Bath*

MALARIOUS FEVER.

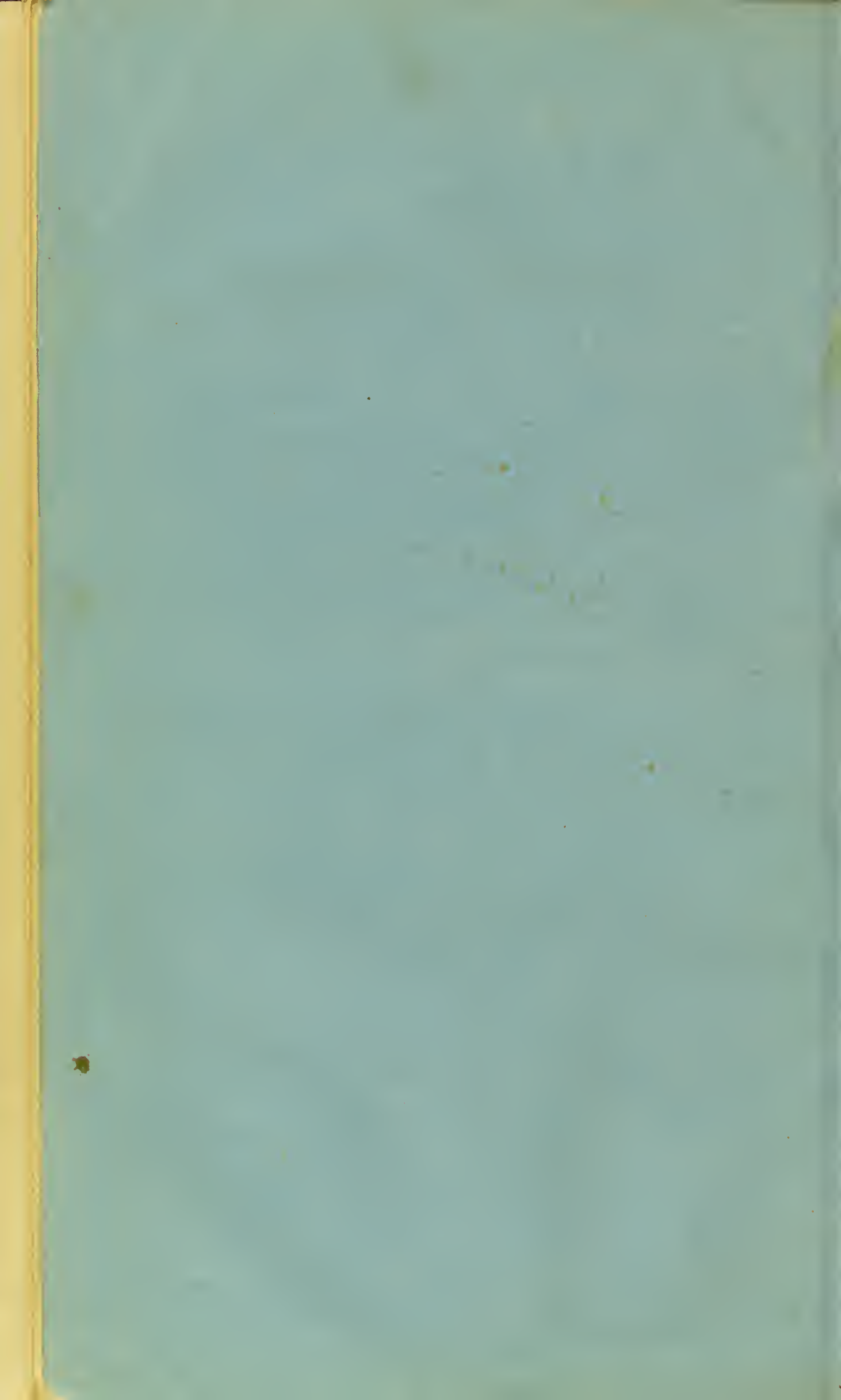
BY E. HARE, ESQ.,

INSPECTOR GENERAL OF HOSPITALS, BENGAL.

CALCUTTA:

O. T. CUTTER, MILITARY ORPHAN PRESS

1864.



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SONS

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ERRATA.

Page 14, last line, omit "these."

„ 28, line 17, from the top, read De Haen for "Dr. Haen."

„ 35, line 28, read bad for "bed."

„ 54, the last word, read its for "to."



MALARIOUS FEVER

BY E. HARE, Esq.,

DEPUTY INSPECTOR GENERAL OF HOSPITALS, BENGAL.

The result of the experiment which I performed in the General Hospital, Calcutta, by order of Lord Dalhousie, in 1849-50, was published March 31st, 1851, by the Medical Board, and a copy sent to every Medical Officer in the service. Lord Dalhousie, on the successful conclusion of this experiment, requested the Commander-in-Chief to give me a European Regiment, with which I might continue my experiment on the treatment of Fever and Dysentery. I was accordingly posted, while still an Assistant Surgeon, to the charge of the 1st Bengal Fusiliers, and my treatment was very soon put to an extreme test, on the Regiment being sent on active service to Burmah during the war. My treatment was again most fully tried during the siege of Delhi, in the mutiny of 1857, when my Regiment, the 2nd Bengal Fusiliers, was encamped in tents before Delhi, during the hot weather and rains. I left my European Regiment in 1861 for England, and since my return to India, having been promoted to Deputy Inspector, and my experimental practice thus terminated, I have obtained the returns of the mortality of my two Regiments, during the nine years I was in charge of them, and finding it to be for Fever* one death in every 211·19 cases treated, or less than half per cent. mortality. The usual mortality† amongst European Soldiers in the Bengal Presidency having been for Fevers, one death in every 32½ cases treated, and for the European Regiments in Calcutta, (see Martin 1861, p. 88,) 1 in 30½ Fever cases treated. I think it my duty to state more fully

* See Table No. 5 at the end of this Paper.

† See Dr. Norman Chevers's Table, prepared in the Office of the Medical Board, for his very valuable review of the Means of preserving the health of *European Soldiers* in India.

than I have yet done to the public, the treatment which I now adopt for Fevers caused by the exhalations from the soil, usually called Malaria.

The importance of even a slight improvement in the treatment of diseases caused by this all-pervading poison, can scarcely be exaggerated: and any change for the better will, no doubt, gradually spread to other countries, and save its percentage of mortality over nearly the whole habitable globe. For in Europe and America, as well as in Asia, is this scourge of the human race severely felt. And it is remarkable that man is its chief victim, for animals do not seem affected by it. Also from every 100 deaths occurring among the European Troops in India,* 23, or nearly one-fourth, have been occasioned by *Malarious* Fever.

Before the discovery of Bark, the treatment of Malarious Fever seems to have been uncertain, and like that of all other Fevers at present, for the poisons of which we have no direct antidotes. Practitioners sought to palliate the symptoms only, as they arose, and no doubt the mortality was very great, for Dr. Johnson says, page 29, that one-fourth of the crews of the Shipping at Calcutta fell annually victims to it. From the strong and very peculiar tendency of the Malarious poison to cause engorgement of the venous system, of all the abdominal viscera, the Liver suffers with the rest, and being the largest, its derangement attracted the greatest attention, and the Fever was called almost universally in its worst forms Bilious *Remittent* Fever; its tendency to alternate paroxysms and remissions being another most prominent character.

This Fever, too, causes so much stagnation of the blood in the venous system of the abdomen, that the blood often partly loses its vitality, even before the death of the patient, and thus causes symptoms of putrescence before, and very rapid decomposition after death. The older physicians, therefore, well named the disease putrid Fever. It is true that, in modern days, we do not see these putrid symptoms, on account of our improved method of treatment, but at the General Hospital, Calcutta, and in Burmah, I have had neglected cases brought to me, who had not been treated till the last stage, and I am quite convinced, from watching them, that were we now as they were then, without Calomel

* Martin, p. 94.

and Bark, we would often witness such cases, and think the older Physicians quite justified in naming the disease putrid. As also the engorgement of the Liver, and consequent derangement of all its secreting functions, causing dark offensive stools, and a yellow tinge of the body, justified the epithet Bilious.

Till the introduction of Bark, the chiefly disputed point in practice seems to have been blood-letting; some looking to the primary congestive stage, other to the putrescent. When Bark was discovered, it found great opposition from the former, and strong support from the latter party: and how difficult it must have been for them to decide from their results, the following remarkable passage will show:—

Dr. Raleigh, late 1st Assistant to the General Hospital, Calcutta, says in his book on Dysentery, page 65—"Some years since, I had the curiosity to examine some of the old diaries of the General Hospital, from the earliest dates that I could decypher, when the practice followed in Dysentery and Fever was as contrary to that of the present time as it is possible to conceive it to have been. In those days, of 238 cases of Dysentery treated from 1797 to 1799, 127 died, or very nearly one-half; now it does not exceed from 10 to 14 per cent. But with Fever how is it, these mouldering records prove," he says, "that the deaths under a treatment of Bark, Opium, Porter, Wine, Brandy, and Meat, viz., Mutton, Roast Fowl, and 6 to 16 glasses of wine daily, do not exceed that of the present era of bleeding and starvation." Bark at that time was used only in small inefficient doses, as an antiseptic, and the extremes of diet produced nearly similar results.

Cinchona Bark was introduced into Europe in 1639, and we find notices of its use in 1751 and 1757, by Cleghorn and Bogue; but there is no trace of its having been used then in any form of Fever but ague, and on the theory of its antiseptic virtues, it was combined with camphor, wine, and arrack, in correspondence with their theory of the putrid nature of Fever. Dr. Lind, however, in 1765, evidently had advanced considerably, and trusted to the *antidotal* power of Bark only, in the cure of Ague, without combining it with other supposed antiseptics. "In the proper administration of Bark," he says, "the cure of agues may be said entirely to consist; other remedies seldom become necessary, unless

“to prepare the body for this, to alleviate particu-
 “lars, or to give relief in cases where the Bark cannot be
 “used. When the ague was stopped by the Bark, after the
 “first or second fit, as in my own case, and those of 200 of
 “my patients, neither a jaundice nor a dropsy occurred.
 “When a dropsical patient suffered a relapse into the ague,
 “which frequently happened, there was an absolute necessity
 “for putting a stop to it by the Bark, and in above 70 such
 “patients, I never observed any other than the most bene-
 “ficial effects to have resulted from it. I never prescribed
 “Bark till the patient was free from the Fever, and then,
 “without regard to a cough, or any other chronic indisposi-
 “tion, I ordered it to be given in large doses. I have given
 “the Bark in every circumstance attending Intermittent
 “Fevers during their remission, but never gave it during
 “the fit.” In another passage, page 123, Dr. Lind explains
 more fully why he avoided giving Bark during the paroxysm.
 He says, “Bark generally excites sickness and vomiting if
 “given during a paroxysm of Fever. A desire to administer
 “early this great specific, sometimes led to this practice, but
 “nothing was gained by it, as it was almost always rejected
 “by the stomach. Nay, there was always some danger of
 “raising a disgust in the patient to the medicine, which might
 “continue during the remission.” He says also, page 121,
 “By emetics the stomach is rendered incapable of receiving
 “the Bark, the only medicine we are yet acquainted with,
 “that possesses power to stop the progress of the Fever.
 “In all cases a vomit ruffled and fatigued the patient more
 “than a purge, without procuring equal relief, and if a vomit
 “were given during the paroxysm of the Fever, it was
 “generally considered necessary afterwards to give a purge,
 “before the Bark could be administered, and thus a whole
 “remission was often lost;” and page 94, “If there be sickness
 “and vomiting at the beginning of the disease, chamomile,
 “tea, or warm water are sufficient to cleanse the stomach.
 “The stomach is also relieved by a purgative glyster. In
 “this way the vomiting is often quieted, and the stomach
 “enabled to retain the Bark.”

Dr. Lind also tried to disguise the Bark by mixing it
 with milk, coffee, wine, &c., “that it may sit easy on the
 “stomach, and no time be lost. By these expedients the
 “stomach is reconciled to the medicine, and enabled to receive

“a larger quantity and retain it better.” “When the method of cure,” he says, “laid down above, is carefully put in practice from the beginning, it will, in many cases, prevent a return of the Fever. In general, however, a sufficient quantity of Bark cannot be given in the first remission, nor is there time for it to produce its effects upon the body, so as to prevent a second paroxysm.”

Of the second paroxysm he writes—“The second remission, as well as the first, is to be employed in the administering Bark freely. In this way above two ounces may in general be got down before the Fever returns, and this will in most cases be sufficient either to prevent entirely the next accession, or so far break the force of it as to render it void of danger.”

The above treatment is as perfect as it could be for bad intermittents before the invention of Quinine. It was simple antidotal treatment, for he gave as much of the “specific” as the stomach could bear, without watching symptoms, or placing any reliance for the cure of the Fever on other medicines, bleeding, emetics, or even purgatives, which last he only gave to prepare the stomach for the Bark, and soothe it to receive, without vomiting, as much of the woody fibre as possible. Dr. Lind, however, never expanded his views or reasoned that the “specific” which would cure one form of malarious Fever, viz., the intermittent, might also cure another, the continued type, and thus he did not extend the utility of his valuable antidote to all the symptoms caused by the one malarious poison. Some practitioners, however, of about the same date, did so, though it may seem to us who have Quinine almost impossible to suppose that enough of the bitter saw-dust could be given or retained on the stomach of a patient with bad Remittent Fever before a remission occurred.

Dr. Hunter says, page 86, “No disease requires more speedy assistance, for the efficacy of the medicine (Bark) employed depends, in a great measure, on its being given early. The disease gains strength by repeated attacks, and is fatal, or materially injures the constitution. To wait for any crisis would be time irrecoverably lost.” He trusts also simply to his antidote, without caring about particular symptoms, for he continues,—“After a purgative,—it is of no great consequence what purgative is given.—A little Epsom Salts,

“Jalap, or Rhubarb will do,—after a few stools have been procured, commence immediately to give Bark;” and he gives the purgative, he says, only “to make the Bark sit easy upon the stomach, for clearing out the bowels; quiets vomiting, and moreover causes a quicker absorption of the Bark into the system.”

Dr. Clark,* who practised in Calcutta at the same time, 1768—1771, boldly adopted the antidotal treatment to its full extent. He says, as soon as the intestinal tubes have been thoroughly cleansed, the cure must *entirely* depend upon giving the Peruvian Bark in as large doses as the stomach will bear, without paying any regard to the remissions or exacerbations of the Fever.

That the reader may more fully understand what I mean when I speak of symptomatic (or treatment of symptoms), and the antidotal as instanced above, I will here quote a passage from our latest† authority on the treatment of the same disease, Bengal Remittent Fever, for the contrast is most remarkable.—

Page. 322.—“By him who would conduct his patient with safety through the troubles and distresses of a Tropical Remittent Fever, two preliminary conditions must be settled. He must first select his means of cure with judgment, and apportion them carefully.——In Fever, above all diseases, we must admit, that he who is able to distinguish what can be done and what cannot be done, is the true physician. But how great and difficult his task! He must measure his means to the wants of nature, doing neither too much nor too little, carefully avoiding what ought not to be done. Before proceeding to the cure of this formidable disease, it is always necessary to observe the age, length of residence in India, the constitution, whether plethoric, bilious, or anæmic; for on these conditions must rest much that concerns the nature, amount, and persistence of our remedies. The physician must likewise enquire, minutely and carefully, into the duration and actual stage of the Fever, that is, whether the actually existing paroxysm be at its accession or decline. These are the first and more immediate points for consideration, for by the daily recurrence of these

* Quoted by Lind, p. 5—126.

† Sir R. Martin, Edition 1861.

“paroxysms, changes take place in the relative balance of
 “power in the several functions, nervous and vascular, which
 “render a proportionate modification, in our more active
 “means of cure, necessary to successful treatment. The
 “means which would be salutary within the first few days,
 “cannot be used later with the same effect, or to the same
 “amount; and so again the means to arrest Fever and save
 “life, if applied at the accession of the paroxysm, would in-
 “duce a dangerous collapse, or even destroy life if applied at
 “the stage of its decline or towards its termination. After
 “these preliminary cautions, the reader will be prepared to
 “enter into the general details of remedies, in their usual
 “order of application, into a consideration of their respective
 “physiological actions, into the preference to be given to
 “some of them, and their application to particular uses. It
 “is of great importance to arrange such means as are pri-
 “mary, so as to separate them from such as are but secondary,
 “or of minor consequence to the cure. For this purpose I
 “have marked under their respective heads the various means
 “in use for the treatment of Remittent Fever, very nearly
 “in the order of their importance, and I would here observe,
 “that whoever pretends to encounter Remittent Fever with
 “a mind unprepared, incurs a fearful responsibility—the suita-
 “ble education and training cannot here be postponed till
 “the emergency arises.”

There is, however, undoubted and overwhelming evidence
 that the simple antidotal treatment of Fever by Bark, des-
 cribed above, was very successful. For Bruce, a man of uni-
 versal knowledge, when he went to Abyssinia, selected Bark as
 the only medicine he required, and he cured the King's and
 Prime Minister's sons by it, of severe Remittent Fever, giving
 the Bark in large doses from the very first, (vol. 2, page 179,) and
 thereby secured their favor, and permission to remain in the
 country. The Jesuits, too, were greatly indebted for their
 success in China, and other parts of the world, to their Jesuits'
 Bark, as it was called.

The celebrated Robert Jackson, 1778, page 315, says, he “felt
 “warranted in saying that the Peruvian Bark has every right
 “to be considered as a specific in Ague and Fever, while it
 “is totally free from the imputation of occasioning Dysentery,
 “Dropsy, or viscerai obstructions. These complaints are
 “always most frequent where the remedy has been most

“ sparingly employed ; 3 or 4 oz. seldom failed to check the
 “ progress of the most formidable Fevers of America, while
 “ one or two ounces frequently did not produce any sensible
 “ effect. Being perfectly convinced of the truth of this ob-
 “ servation, I generally gave Bark, in doses of 2 drams, and
 “ where danger appeared to be threatening, the doses were
 “ increased to half an ounce, or even more. In some cases
 “ of obstinacy, indeed, accompanied with a sluggishness of
 “ constitution, I observed no other rule in the quantity than
 “ such as proved disagreeable to the stomach, or excited a
 “ tumult in the system. The method was generally successful,
 “ and I may observe in general, that 2 ounces taken at 5
 “ or 6 times, and in the space of 8 or 10 hours, were often
 “ more effectual than double the quantity, in small doses at
 “ long intervals.” Dr. Jackson,* to assure his readers of
 the impunity with which large doses of the Bark could be
 exhibited in certain cases, states that he had himself fre-
 quently taken an ounce at once.

Dr. Lind expresses his convictions in favor of Bark in still
 stronger terms, page 257—“ Although I annually prescribed
 “ upwards of 140 lbs of Bark, I never observed any bad effect.”
 Page 122, “ the prejudices that formerly existed against the
 “ Bark are no longer in being. They were founded on the spe-
 “ culations of the learned, but even with the vulgar they are
 “ now extinct. Any attempt to prove that the obstructions
 “ of the viscera are the effects of the disease, and not of the
 “ medicine, would at this time be deemed superfluous. The
 “ greatest, indeed, and the only evil arising from the Bark,
 “ that has fallen under my observation, has been to excite sick-
 “ ness and vomiting during a paroxysm.”

It was however this very evil of vomiting, combined with
 new theories on inflammation, caused by the publication at
 this time of Dr. Baillie's work on morbid Anatomy, which
 suddenly, by the agency of Dr. James Johnson's classic writ-
 ings, displaced Bark from general practice, and substituted
 bleeding and calomel. I can only give here brief extracts, but
 Dr. Johnson's book is most interesting, and well worthy the
 most careful study. Calomel, too, being tasteless, and easily
 given, no doubt must have been greatly preferred, by both
 patient and physician, to such a bulky nauseous powder as Bark.

* Medical Annals, No. 14, page 339, by Dr. Ewart.

Page 107.* Dr. Johnson arrived in Calcutta in September 1804. Page 45—"and Fever," he says, "was making prodigious havoc among the ships' crews at Diamond Harbour. Nor were we long exempted from its visitation. I thought myself fortunate in having the works of two celebrated authors Clark and Lind, containing a full account of this Fever, drawn from personal observation on the spot. I accordingly studied them with great attention. In short, I was quite ready to grapple with this Hydra disease, and show the power of medicine over this scourge of Europeans. His first case, page 47, was a young healthy Sailor, who was seized with the usual symptoms of this Fever. An emetic was prescribed, a perspiration broke out, and I now with an air of confidence began to throw in the Bark, quite sanguine in my expectations of soon checking this formidable disease. But, alas! my triumph was of short duration, for in a few hours the Fever returned with increased violence, and attended with such obstinate vomiting, that although I tried to push on the Bark through the paroxysm, it was all fruitless, for every dose was rejected the moment it was swallowed, and I was forced to abandon the only means by which I had hoped to curb the fury of the disease. The other methods which I tried need not be enumerated, for they were temporary shifts, calculated in medical language to obviate occasional symptoms. The plain truth was, I knew not what to do, for the sudden and unexpected failure of that medicine on which I had been taught to depend, completely embarrassed me, and before I could make up my mind to any feasible plan, my patient died on the third day of his illness, perfectly yellow, vomiting to the last a dark fluid resembling vitiated bile, and exhibiting an awful specimen of the effects which a Bengal Fever is capable of producing in so short a period, on a European in the full vigor of manhood. On opening the body, I was surprised to find the Liver so gorged with blood, that it actually fell to pieces on handling it. Indeed it appeared as if the greater number of the vessels had been broken down, and almost the whole of the interior structure converted into a mass of extravasation. In the Brain were marks of turgescence in the venous system particularly, and more than

* Dr. Jas. Johnson on tropical climates, 3rd Edition.

“the usual quantity of lymph in the ventricles.” Dr. Johnson did not try Bark again, but resorted to bleeding for his next patient, and soon settled his practice of bleeding and evacuations “with a high hand” and scruple doses of Calomel, with injunctions to produce rapid salivation.

It was certainly very unfortunate that Dr. Johnson's first case should have been one of *the* cases for which Bark must fail, for the simple reason that it is vomited, and therefore could not be given. Had he continued with more patience the Bark treatment in other more favorable cases, he would soon have found that all that Clark and Lind had said of Bark was true. However, there can be no doubt, that such cases as the above did constantly occur, and that they all died under the Bark treatment, which in fact was then no treatment at all, for the Bark was all rejected. But Calomel and bleeding were always applicable, would constantly give great relief in such cases, and cure no doubt numbers of them. The vomiting and the uncertainty of Bark in these severe cases, where it was most required, were known to all practitioners: the failures and dreadful effects of the Calomel and bleeding, had not yet been experienced. The theories of the day too, all ran in favor of evacuations, for supposed bile and inflammations. The congestions almost to bursting of the vessels of the liver and brain, were considered as visible proofs of the necessity of blood-letting, and though this sudden revolution is the most surprising fact in the records of medicine, we can see evident and sufficient reason for its occurrence. And writing as I do now, in the full knowledge of the grievous errors and fearful misfortunes inflicted on Europeans in India by Dr. Johnson's system, I confess had I lived in his time, before Quinine had been discovered, and before the evils caused by salivation and blood-letting, and the failures generally of this system could be known, without doubt I would have adopted it, and plumed myself on my just reasoning in doing so. It will be very instructive, however, if we trace more carefully, from Dr. Johnson's admirable Book, his own reasoning more in detail, for there is a marvellous substratum of truth in it, which his powerful mind seized, and which supported him through his practice.

Dr. Johnson first commenced with bleeding. He writes, page 49, “The impression on my mind by the above dissection, and the perusal of Dr. Clarke's case, Henry Pope,

“where the determination to the liver and brain is perfectly
“evident, determined me to try venesection. I had now
“several down with Fever, and I must confess that it was
“with a trembling hand and palpitating heart that I first
“opened a vein, expecting to see the patient every instant die
“under my hands. He did not die, however; nay, he seemed
“evidently relieved, but the bad symptoms soon returned,
“and bleeding was repeated with brisk evacuations. He
“recovered. I now carried the evacuating plan with a high
“hand, and with much better success than I expected. For-
“tunately for my patients, the great majority of them were
“fresh from Europe, and high in health and strength. These
“recovered wonderfully after bleeding and evacuations, though
“not always. But there was on board a class of men, whom
“we had pressed out of ships on their return from India,
“who had experienced not only the influence of the climate,
“but of depressing passions, arising from hope deferred.
“These required a more discriminating treatment. Evacua-
“tions at the very beginning were necessary, but something
“more was necessary to clear the congestions from the head
“and liver. The fluids here were too stagnant to drain off of
“their own accord; even when a sluice was opened, they re-
“quired propulsion.”

This reasoning with his own case, and the discovery which he made of the fact, that scruple doses of Calomel act like a charm in alleviating the vomiting in Fever, induced him to adopt salivation, and scruple doses of Calomel, as the something more, and the propulsion which was necessary.

His own case was thus, page 198.—“As very few weeks
“after my arrival in Bengal, I landed a few miles below
“Kedgerree, for the purpose of shooting and seeing the
“country. The day was excessively hot, and we waded
“through marshes, jungles, and paddy-fields under the
“broiling sun, till we were fairly exhausted. That night
“I felt slight rigors or chills, alternated with flushes of
“heat, but in the morning I got up as usual, and con-
“cluded that all was well. At dinner I had no appetite,
“and soon afterwards felt uneasiness in my bowels. As the
“evening advanced, I had frequent calls to stool, with grip-
“ing and some tenesmus, nothing coming away but mucus.
“Fever now came on, my skin became hot, dry and parched,

“and by 11 o’clock at night I could scarcely leave the com-
“mode. The misery of that night will never be erased from
“my memory, and I was often delirious. A medical gentle-
“man visited me, and found me in a very bad way. I was now
“passing blood fast, and the Fever ran high. I was bled, and
“took Calomel 6 grains Opii. 1 grain every 5 hours. This day
“passed easier than the preceding night, but as night closed,
“the exacerbation was great, and I was again delirious. The
“next day I was very weak. The Calomel and Opium were
“now taken every 4 hours, with the addition of mercurial
“frictions. My fever ran higher this day than yesterday,
“with hot, dry constricted skin. As night approached,
“my debility and apprehension of the usual exacerbation,
“brought on an extreme degree of mental agitation. The
“Surgeon endeavoured to cheer me with the hope of Ptyalism,
“for my medical friend held out no other prospect. I sent for
“my assistant, and desired him to give me scruple of Calomel,
“which I instantly swallowed, and found that it produced
“no additional uneasiness, on the contrary, I thought it
“rather lulled the tormina. But my sufferings were great,
“my debility was increasing rapidly, and I quite despaired
“of recovery. Indeed, I looked forward with impatience
“to a final release. At 4 o’clock in the morning I repeated
“the dose of Calomel, and at 8 o’clock, or between 60 and 70
“hours from the attack, I fell for the first time into a
“profound and refreshing sleep, which lasted till near mid-
“night, when I awoke. It was some minutes before I could
“bring myself to a perfect recollection of my situation prior
“to this repose, but I feared it was still a dream, for I felt
“no pain whatever. My skin was covered with a warm
“moisture, and I lay for some considerable time without mov-
“ing a voluntary muscle, doubtful whether my feelings
“and senses did not deceive me. I now felt an uneasiness
“in my bowels, and a call to stool. Alas! thought I, my
“miseries are not yet over. I wrapped myself up to prevent
“a chill, and was most agreeably surprised to find, that with
“little or no griping, I passed a copious, feculent, bilious
“stool, succeeded by such agreeable sensations, that I eja-
“culated aloud the most heartfelt tribute of gratitude to
“Heaven for my deliverance. On getting into bed, I per-
“ceived that my gums were much swollen, and that the
“saliva was flowing from my mouth. I took no more medi-

“cine, and enjoyed the best of health for some time afterwards.”

Such results as these were very common when Calomel was used. I have seen numbers of them myself, both for Malarious Fever, and, as this was, Malarious Dysentery, the result of Fever, from shooting in the swamps of the Hooghly. The alternate remissions and accessions of the Remittent Fever, causing the paroxysms of purging, are very clearly described by him. In this, as in Fever, the paroxysm may be at its height, the patient exhausted and in despair; when suddenly comes on the salivation, and he is safe and well, and feels most delightful sensations.

Doctor Johnson thus found the something which he required, to propel the too stagnant blood in the abdominal viscera, which he could not drain off by bleeding alone. And, as he experienced in his own case, the moment salivation takes place, the circulation is equalised, the skin becomes warm and moist, and the abdominal congestions and resulting purging are removed. The blood passes on again as in health through the capillaries, under the stimulus of the salivation state. The febrile struggles of nature in paroxysms against the congestion cease with their cause, and the patient is well.

Here, therefore, another antidote was introduced, producing the same end as Bark, viz., the solution and relief of the congestions.

For Bark also relieves the congestions of Fever, and thus stops the paroxysms, which are merely Nature's salutary efforts to overcome them. But Bark sometimes could not be given, because it caused vomiting, and therefore failed in numerous cases, which all died. Mercury could be given at all times. Mercury, therefore, appeared to be the best, and experience only could decide between them. That salivation was used as a specific antidote, is evident. Thus (page 51): “Immediately after the operation of the Cathartic, the mainspring of cure must be acted on. For this purpose, 5 to 10 grains of Calomel, according to the urgency of the symptoms, should be exhibited every 4 to 6 hours, till Ptyalism is well raised, when in 19 cases out of 20, I might say 49 out of 50, there will be a remission of all the febrile symptoms and safety secured. It is undoubtedly the *sine quâ non* in the medical treatment of this Fever.”

Put Bark above in the place of Calomel, and you have the exact assertion of Lind, Hunter, and Clark. Put Cinchonism from Quinine in place of Ptyalism, and you have modern experience.

Again, page 52, he says.—“ Once every day then, the dose of Calomel usually given every 4 or 6 hours, should be combined with a purgative, which will be sure to bring down a copious alvine evacuation. This will greatly relieve the oppression and tension of the epigastrium, as well as the headache. Indeed, so striking is the amelioration of symptoms, after these intestinal evacuations, that in two or three instances, I was tempted to follow them up, and try if they might not supercede the necessity of impregnating the system with mercurry. I trode here on tender ground, and I was forced to measure back my steps, and have recourse in the end to that powerful and invaluable medicine; but in one case I was too late. Warned by this, whenever I combined a purgative with the Calomel, I directed a mercurial friction or two to be employed during their operation, to prevent a halt in the pursuit of my ulterior and principal object, Ptyalism.”*

There is no nice calculation here to decide as above quoted, from a Classified List of Medicines, which is physiologically adapted, after careful weighing of the symptoms, to the case in hand. The direction is simple and unique, “salivate,” as it was in the former system “pour in the Bark.” It was therefore purely treatment by an antidote.

Mercury had, however, been constantly used in the treatment of Tropical Fever before Dr. Johnson’s time. Wade, in 1791, wrote strongly in favor of mercurialization, and Dr. Chisholm in 1792. Dr. Robert Jackson says, in 1798, page 293: “Calomel alone, or with the addition of different purgatives, has been long employed as an evacuant in the cure of recent Fever, particularly of the remitting kind, and often with superior advantage; but the discovery of its virtues as a general remedy, and on the grounds of salivation, is due to Dr. Chisholm.”

Milne, 1803, says, page 63: “He first gave 5-grain doses of Calomel, but, at the instigation of Mr. Liddell, he increased the quantity; and ~~these~~ doses I should have shuddered at

* Dr. Martin, p. 382.

“before, became only common prescriptions with me. I now “make it a point of making every one’s mouth sore as early “as possible, with a confidence I was formerly a stranger to.”

Dr. Gibson also strongly recommended mercurialization in Guzerat Fever. It was from these sources, no doubt, that the Surgeon who attended Dr. Johnson, told him to expect salivation as his certain cure. The practice of these men, however, as yet was not usual. The Bark treatment of Lind and Clark was the rule, until Dr. Johnson’s Book and talented Review succeeded in suppressing it, in a very short time, so entirely that it was never mentioned again but as an exploded error, and with surprise that such irrational treatment could ever have been adopted.

The evils, however, of the Calomel system soon began to show themselves. A patient could be salivated generally with apparent impunity once in his life, but residents in India were liable to frequent attacks of Fever, the salivations had to be repeated, and with most deplorable results, old Indians returned to Europe, suffering not from the climate, but with their constitutions broken by Mercury. Before I came to India in 1839, I met several such cases in England. It was found, too, that frequent cases of the worst form of Fever could not be salivated, the absorbent system was so oppressed by the Fever that the Mercury remained inert in the stomach, and the skin also refused to imbibe it. Enormous doses were then prescribed. Chisholm actually gave* 6,000 grains of Calomel in one case. Dr. Haliday’s description of the horrible effects of the mercurial system, in the General Hospital, Calcutta, in 1816, is terrible. He accused openly his colleagues of murder, and they were obliged to appeal to Government, and turn him out of Calcutta. Dr. Haliday states that 13,337 grains of Calomel were expended in the General Hospital in one month. Throughout the Fever Wards, every one had his spitting basin, and in the cases,—and they were far from unfrequent,—where salivation was difficult, and very large doses given, if reaction towards recovery took place, the Mercury lying inert in the bowels was suddenly absorbed, and the most fearful salivation resulted, with sloughing of the mouth, and exfoliation of the jaw. If a patient recovered with such treatment from his first attack of

* Dr. Ewart, p. 341.

Fever, how was he to be treated in his second and third, to which all residents in India are liable? These results of the Calomel treatment at length produced reaction. Inflammatory theories of Fever, however, still prevailed, and no one thought of returning to the use of Bark.

Sir James Amnesley in 1825, first commenced the change in recommending only moderate doses of Mercury frequently repeated with depletion and especially free and continued purging. He condemned salivation theoretically, though if the cases he has published in his Book be examined, it will be seen that in his practice most of his patients had their gums made sore. A gradual reaction, however, took place in favor of depletion. Still, in 1826, Dr. D. S. Young writes:—
 “The great desiderata in the cases of Europeans are to remove congestions by bleeding and blisters, and to establish the mercurial action as soon as possible. To effect the latter indication, I have found the scruple doses of Calomel to answer better than small doses frequently given, which increased the irritability of the stomach, without exciting Ptyalism sooner than by Dr. Johnson’s plan of large doses, which always stopped vomiting. In bad cases I could observe no symptoms of amendment till salivation was induced, and which I practised in almost every case submitted to my care.”

Depletion, however, gradually banished the large doses of Mercury. In 1825 Dr. R. N. Barnard says:—“The treatment I was in the habit of adopting, when called to a patient attacked by Fever, was immediately to open a vein, and suffer blood to flow *ad deliquium*. From 25 to 40 oz. is the quantity usually required, but I had no hesitation in going beyond this, when circumstances demanded it.” With Dr. Twining, however, the bleeding system culminated. In 1834 he says:—“Blood-letting is the most important and most efficient remedy which we possess in this Fever; and employed early, it not only immediately arrests the dangerous symptoms, but it prevents embarrassment in the future treatment and renders other remedies more effectual and certain. Europeans of unimpaired stamina, suffering from a severe attack, should be bled from lbs 1½ to lbs 2 as soon as possible. The blood-letting should be performed when the patient is in the recumbent posture; 12 grains of Calomel and as much Extract Colocyath

“ should be given soon after the bleeding, and followed by
 “ Infusion of Senna and Salts in 6 hours. If there be much
 “ thirst, with any degree of dryness of tongue, 2 grains of
 “ Tartarised Antimony are to be dissolved in a pint of water,
 “ and a table-spoonful given every hour, and not much
 “ other drink. The Solution of Tartar Emetie, if used
 after the bowels have been cleared out, may be estimated as next in efficacy to the lancet. In four hours after the first bleeding, if the Fever continues, the vein is again to be opened, and blood taken to the extent of 8, 12, or 20 ounces. In 8 hours more, if the symptoms be not much abated, a third bleeding should be ordered, but if the pyrexia be moderated, while only headache or morbid heat of forehead continue, leeches are to be applied to the temples: “ For proofs of the extreme,” says Dr. Ewart, “ to which
 “ Twining carried out these principles in his own practice
 “ (see Cases 166, 169, 170, 171, 173, and 175, pages 305 to
 “ 338) and for detailed evidence of the same tenor, in
 “ regard to his treatment of the insidious congestive Fevers of
 “ the cold season (see cases 177, 178, 179, and 180, pages 369
 “ to 383), neither Sydenham in the 17th, Jackson in the
 “ 18th, nor Johnson at the commencement of the 19th century, carried venesection with a higher hand than did
 “ William Twining at the beginning of the 4th decade of the
 “ 19th century, at the unhealthy capital of British India.”

Numbers, in fact, of his patients plainly and evidently, as described by himself, died from exhaustion.

As a specimen* of this depleting system, the following details are given of a bad Remittent Fever. A Mr. B. was bled to lb 2 at 9 A. M., 9th July 1834; at 2 P. M. of the same day lb 2 more were abstracted, and an enema of salt and oil was administered. After sitting in a tepid bath for half an hour, a scruple of Calomel was given; at 9 P. M., 15 grains of Calomel, Opii 1 grain; Extract Coloeynth 15 grains were administered on the morning of the 10th, Castor oil an ounce. At 7 A. M., 16 ounces of blood were taken away, and antimonial wine in camphor mixture was prescribed. At noon 18 leeches

* Indian Annals, No. 14, p. 357.

In the 7th vol., Calcutta Med. and Physical Societies' Transactions, pp. 367—372.

were applied to the right side; at 9 p. m. the patient was bathed in perspiration and a blister was applied to the epigastrium. The patient expired at noon on the 11th. On the *post mortem* examination, all the organs were pallid and blanched. The spleen was gorged with blood, and crumbled to pieces on being handled.*

We are further informed in the report, that in "all the severe cases of continued Fever, venesection was freely and repeatedly employed at the commencement, followed by scruple doses of Calomel immediately, and these by doses night and morning of 10 grains of Calomel and 15 grains of Colocynth, with a purge of Compound Jalap at noon. Leeches and blisters were useful towards the end of the ease."

I will give one other case, showing the results of such practice.

H. Edwardes, admitted August 23rd, having been treated since the 16th for Remittent Fever; he had been bled, leeched, cupped, blistered, and had taken Calomel and Antimony. When admitted he had a weak, fluttering pulse of 130, and irregular in its beat; skin cool and moist. He was restless; and constantly sighing. His bowels were irritable. On the 25th the Fever assumed the typhoid character, sordes appeared on the teeth, and he died on the 26th.†

But it is useless filling my pages with such cases with which all the medical works of this period abound, and which I hope now will cease ever again to recur. The theory which induced Twining to practise his excessive blood-letting, was, he tells us himself, to carry out in Remittent Fevers Mackintosh's plan of bleeding in the cold stage of Ague, in order to catch the paroxysm just as it was coming on, and break it. That bleeding has this effect there can be no doubt. A few ounces of blood drawn from the arm, just as the shivering of the cold stage commences, has a most remarkable effect in breaking the following hot stage; and, if the

* I will note in passing that this most extreme depletion did not remove the congestion of the spleen; or in Dr. Johnson's words (p. 50) "The fluids here are too stagnant to drain off of their own accord, even when a sluice is opened. They require propulsion."

† From the Transactions of the Bombay Medical and Physical Society, Dr. Stowell's Report, see Dr. Morehead, p. 58.

remedy and the loss of blood, required to be repeated for each paroxysm, were not worse than the disease, bleeding in the cold stage of Ague must be considered a valuable improvement. Twining adopted the idea, which was new in his time, and generalised it by applying the same method to check the paroxysms in Remittent Fever. He says:—The recent practice of Bleeding in Intermittents, as recommended by Dr. Maekintosh, not only accords with the acknowledged pathology of that class of Fevers, but seems to bring our system of therapeutics, as applied to them, within the limits of those established principles adapted to the treatment of *other* Fevers. And the Profession is indebted to Dr. Twining for pointing out, in following this theory, the exact time for bleeding in Remittent Fever, to secure its best effects. Before his book, the time for Bleeding had been left very vague, but Twining showed that to bleed with best effect it must be done, not when the paroxysm is at its height, when it would seem to be most peremptorily required, but just when the paroxysm is commencing, and the earlier the better, and particularly never to bleed when the paroxysm is on its *turn* even though the heat and violent symptoms be very distressing. This was a very great improvement. Dr. Martin says, page 349,—“a young Officer was seized with Fever at 11 A. M. He was bled at 4, and he was dead at 7 P. M. The same operation practised at noon would probably have put an end to the Fever.”

And I have seen myself a strong young man bled when the skin was still hot, with headache and bounding pulse, yet when the accession was over the turning point, I saw that man sink from a moderate bleeding, and die exhausted in a few hours. He never rallied, for the exhaustion produced by the paroxysm, combined with the bleeding, destroyed him without remedy. In fact, it is not the principle itself which is wrong, but it is the necessity of repeating these bleedings, as the accessions recur, which makes it so injurious. The fact itself is correct, that we can break a paroxysm by bleeding in the cold congestive stage; but breaking one paroxysm does not prevent the recurrence of others, and the repetition of the bleeding is ruinous. Bleeding in the cold stage is helping nature to restore the equilibrium of the circulation, but in the hot stage, it is hindering her, just as she is striving for the mastery; as also bleeding in the

commencement of the sweating stage or remission increases her exhaustion.

A remarkable case in proof of this effect of bleeding, in relieving the oppressed circulation, is given by Mr. Bland in the *Australian Medical Journal*, January 1861. He describes a patient bitten by a deadly species of snake, (the gold coloured or yellow snake.) "The patient had an insupportable feeling of oppression in the left side of the chest, perhaps the direct action of the poison on the heart, or more probably on the blood itself. V. S. to 16 ounces produced the most immediate and beneficial result, not only in the removal of the oppression, which evidently but for this timely relief must have been rapidly fatal, but with almost immediate and complete recovery of the patient." It is thus, no doubt, that a small bleeding will often restore the circulation in the collapse of Cholera, in drowning, or in any other form of Asphyxia. It relieves the oppressed heart for the moment, and thus enables it to restore the circulation. It would do this also in many cases of Remittent Fever, but here the paroxysms recur, and your patient cannot bear the remedy again. I have often theoretically contemplated the possibility of advantage in bleeding a strong young man, if he were sent to me just as the paroxysm was coming on; and I thought I had not time to check it with Quinine, but in practice I have never applied it. It is not necessary. Quinine will do all you require, and the anæmic tendencies of Malarious Fever, as I will hereafter explain, are so great that, being unnecessary, bleeding is never justifiable.

It is curious, however, to trace with what false theories real improvements are often introduced. Dr. Twining says:—"In the early stages of Intermittent Fevers, disorder of the digestive organs is co-existent with congestion of the brain, and attended, in some cases, with tolerably distinct evidences of inflammatory condition, either in the cerebral membranes or in the brain itself." Dr. Twining, therefore, really thought that he was curing inflammations in thus bleeding for Ague. Doctor James Johnson's reply to this passage is characteristic:—"What kind of inflammation must that be which explodes as it were the moment the clock strikes a particular hour, and this for days and weeks together? What kind of inflammation is that, which every second day terminates in profuse perspiration from head to foot, and yet is

“renewed after an interval of 48 hours, with the symptoms
 “as before, and so on? Do we see real and unequivocal
 “inflammations pursue this course? Never.”

Fortunately, however, for Dr. Twining's patients, Quinine had just then been discovered, and he used it, though with extreme caution, as a tonic, in the last stages of his Fever eases. In the Remittent Fever of Bengal, he says:—
 “The abstraction of blood at the commencement of the first
 “and second exacerbatation, was invariably of benefit, (and he
 “gave), as soon as the violence of the pyrexia began to abate,
 “Sulphate of Quinine in 2-grain doses for four doses, and in the
 “severe forms 4-grain doses the next morning, to be repeated
 “at 8 A. M., for the purpose of anticipating and preventing
 “the accession of another paroxysm.”

Again he writes:—“In Remittent Fevers, we depend
 “chiefly on active remedies at the commencement of
 “those forms of the disease which come on abruptly in
 “plethoric persons, and are attended with signs of local inflam-
 “mation and high arterial action: for life often depends on
 “the management of a single paroxysm, and two or three
 “large doses 4-grain, of the Sulphate of Quinine.”

It was this introduction of Quinine, however imperfect, which prevented Dr. Twining's practice from being a wholesale slaughter.* His work is full of cases, showing how rapidly his patients recovered, if not too far depleted, before he would allow them Quinine.

I arrived in India very soon after Dr. Twining's death, and found the treatment of Fever in India very much as Dr. Twining left it. Large depletion, however, was gradually becoming unpopular, and Quinine, though with repeated cautions against using it before bleeding and purging had brought on a full remission, was very generally given in doses up to 5 grains three or four times a day; and even a dawning of the full truth appears, in 1841, in the following passage from the valuable work by Sir R. Martin, the most enlightened and successful of our Calcutta Practitioners at that time, page 123:—“Tropical Fever, Dysentery, and Hepatitis
 “are produced by the action of specific causes, and such
 “diseases are nowhere remedied by blood-letting and purging

* Count Cavour, in Italy, seems to have fallen a victim to similar bleedings for Fever.

“merely, as in the instance of inflammation. Yet bleeding
 “here, as in Dysentery, is the standard remedy, subject to age,
 “constitution, and length of service in India. It preceeds all
 “other management in order of time and in point of im-
 “portance. I believe this to be the general view of it taken
 “by the practitioners of this city, and it is but common
 “justice to say, that the value of this most powerful of all
 “means was first emphatically urged on the Indian Surgeon
 “by Dr. James Johnson, and it is to him we owe that blood-
 “letting has become a systematic part of our treatment.
 “Subject only to the limitations already stated, early and
 “copious bleeding, and practised at the very outset of the
 “stage of re-action, is very generally necessary in the
 “severer forms of Bengal Remittent Fever: then full doses
 “of Calomel, short of producing salivation, with saline
 “purgatives in the intervals. If the disease does not
 “now yield, but, on the contrary, if the secretions
 “become of a watery character, or altogether suppressed,
 “and the paroxysms recur at shorter intervals, or with
 “increased severity, leaving but imperfect remissions, then
 “there is imminent danger, and inflammation, or acute con-
 “gestion in some important abdominal or other organ, may
 “be more than suspected. For this, in addition to topical
 “bleeding and cold to the head, when the seat of the disease,
 “Mercury in small repeated doses with Antimonials, must be
 “given, so as mildly to affect the system; it is the only
 “known means of saving the patient, by anticipating the
 “destruction of some organ essential to life. It here becomes,
 “in the apt words of Dr. Robert Jackson, a remedy of
 “necessity.

“Where the remissions, on the other hand, are well marked,
 “Quinine should be given and in full (3 to 5-grain doses,)
 “without waiting for everything. Some practitioners
 “recommend that before this drug is used, we obtain
 “previously a clean tongue, natural secretions, and the
 “absence of all heat of skin or local affection. I believe
 “this to be a very dangerous practice.*

“If we are to wait for everything, we shall often wait too
 “long, or till it is too late. I have always administered
 “Quinine in the more favorable cases now stated, in dis-

* See 2nd Edition, 1861, p. 338.

“ regard of certain local abdominal complications (those of
“ the head in general should exclude it), believing that if I
“ arrest a paroxysm, I do greatly more towards the cure at
“ large than Quinine can possibly do harm to the local
“ affection.

“ In the more favorable cases last spoken of, it rarely
“ happens that topical bleeding, purgatives, and mild Mer-
“ curials, with Quinine during the remissions, fail in con-
“ ducting the patient to safety.

“ The treatment of ardent Fever of the hot season in
“ Bengal consists of active blood-letting, to the extent of
“ allaying the vascular disturbance, cold affusion and con-
“ tinued cold to the shaved head, brisk purging, followed by
“ Calomel and Antimony, until the secretions are rendered
“ healthy, and relief obtained. In this Fever, as well as that
“ of the cold season, the skin is harsh to the touch, com-
“ pacted, hot and dry, so as to admit of a more free use of
“ Antimonials and Diaphoretics than we find requisite in the
“ Fever of the rains. Where remission takes place, provided
“ the head be relieved, Quinine must be had recourse to, for
“ the purpose of preventing recurrences.”

But there were a large class of cases, and these the most fatal, where no remissions occurred, and where there *were* head symptoms, for these, therefore, as yet there was no remedy, but Mercury, for Quinine was excluded by wrong theories.

I have given these quotations the more fully, because they represent the most enlightened practice of India, of all the best Medical Officers, during my first years in India. And the same doctrines are still advocated by Dr. Martin, in his last Edition, 1861. They were indeed universal till 1850, with the exception of some practitioners using Calomel more freely, and Quinine with more caution than Dr. Martin.

I arrived in India in July 1839, and till May 1843 was occupied in the Cabul Campaign. I had the advantage there, however, of practice with Her Majesty's 44th Regiment, whose Surgeon, my kind friend Dr. Harcourt, taught me the same principles of practice as Dr. Martin's. In 1842 I was sent to Segowlie, on the borders of the Nepaul Terai, the most deadly in India, and there remained for four years. I was called to a distance on one occasion to see a Medical Gentleman with Cholera. He died, and left me a valuable Medical Library in which I found the now scarce works of

Lind and Hunter. Their practice was new to me, and I read them with eagerness. I had seen enough of the standard practice to be dissatisfied with it, especially in some recent cases I had treated of the Terai Fever. They all died, no remission took place, there were head symptoms, and I durst not give Quinine. In fact it was so utterly forbidden by all authorities, that it never occurred to me to give it. I tried to salivate, but the Fever was so active that my patients were dead before the Mereury had time to affect them.

It then struck me as remarkable that, since the discovery of Quinine, no one had tried it in the same way, as Lind and Hunter had used Bark, from the dread of its increasing congestion and inflammation. And a case quite hopeless under the common treatment, soon offered itself to me, and I determined to try Quinine.

I was sitting in my room one morning, when my servant came in haste to tell me that some Natives had brought a European very sick from the Terai, and I found a young lad of about 20, lying nearly insensible on a Native bed. The Natives said that he was travelling on a pony in the Terai, had fallen off insensible in their village, and fearing he should die there, and cause suspicion, they had brought him to the nearest Doctor.

I immediately mixed a scruple of Quinine in some wine, and by giving him a tea-spoonful at a time, made him swallow it. I repeated it every four hours three times that day, early in the morning he was sensible, and I gave him another dose, and some arrowroot and milk. He took the same doses throughout this day with some soup, and the next day, to my delight, he was out of danger, and to my surprise, he had taken $2\frac{1}{2}$ drams of Quinine in 48 hours, and without much inconvenience.

The poor lad was a deserter from the Regiment at Gazeepore, and he had thus wandered in the deadly Terai, till he nearly perished. He returned to his Regiment quite well.

I soon had plenty of other cases to try my new treatment on, and among the rest, the Resident of Nepaul, who foolishly passed through the Terai in the unhealthy season; also a number of Indigo Planters in the district, whom I was called to attend for bad Fevers.

Having thus accumulated sufficient evidence, in November 1847 I published at Delhi a Pamphlet styled—"Hints,

on Fever and Dysentery," which I was soon gratified to find had caused a great sensation.

Dr. Ewart says:—"This pamphlet, Hare's Hints, appears to have taken the profession by surprise, and created a great sensation throughout the length and breadth of the Company's Indian Possessions, strong proofs of the startling novelty of the propositions therein advocated, at least among the general body of practitioners in India at that time."

Dr. Mackinnon, a very able Surgeon, and of large experience, in his work on tropical diseases, just then published, 1848, says, page 207 :—"The manner of using Quinine being here under consideration, I am reminded of having very lately perused a pamphlet by Mr. Assistant Surgeon Hare, entitled 'Hints for an improved method of treating Fever and Dysentery.' Regarding Fever, if the doctrine in that pamphlet be correct, the profession in India, and I, as one of it, have been very blind and unobservant. In Fever we have been watching, and in Remittent Fever, with the feeling that life or death hung upon it for the time when we could begin Quinine. Now we are informed that waiting is in every point of view a loss of time; that Quinine may be given safely in the highest climax of the Fever, and that it will prevent the congestions and complications which end in death."

Dr. Edlin's Medical Journal then commenced the controversy, and I wrote several articles for him. At last the sensation became so great that the Medical Board at Calcutta recommended Lord Dalhousie to let me try an experiment in the General Hospital. Lord Dalhousie sent for me, I explained to him my plans, and was ordered to go to Calcutta.

The arrangements for my experiment,—as settled by the Medical Board in correspondence with Lord Dalhousie,—were that one year should be given me for the trial, to comprise each season of the year. A separate ward in the Hospital was made over to me, and a staff of assistants, and all the Fever and Dysentery cases which presented themselves at the Hospital were sent to my ward without selection. I will omit, however, all further mention of my Dysentery experiment, because I propose writing on that subject hereafter.

* Indian Medical Annals, No. 14, p. 367.

It was on the 1st November 1849 that my experiment commenced. In addition to my ward in the General Hospital, I was ordered to do duty, as an Assistant, under the Surgeon of Her Majesty's 70th Regiment. This Regimental Hospital adjoined the General Hospital, and a ward was given me there also, and all the Fever and Dysentery cases from one Wing of the Regiment were sent to me, and those of the other Wing were treated by the Surgeon himself. As I was afterwards told, the Medical Board placed more confidence in this part of the experiment than the other, for they had a direct comparison of mortality, with men of the same Regiment, under the same rules and diet, the same place of residence, during the same year, and in all circumstances were the patients alike, except their mode of treatment. The Surgeon too had the daily supervision of my ward, while Sir James Thomson inspected the other, and every grain of medicine I prescribed was recorded. In one respect, in the Regimental Hospital, I felt I was under considerable disadvantage, for the Surgeon had the upper story, and I the lower, which any one who has treated Fever patients in swampy Calcutta will confess to be a great disadvantage, and besides I found that the Surgeon, inspecting as he did the efficacy of it in my patients, made a far more early and liberal use of Quinine than had ever before been his custom, though he could not suddenly change his practice and give Quinine in the wholesale manner I did. However, the result at the end of the year was, that the Surgeon treated 279 cases of Fever with 4 deaths, while my result was 292 cases and 2 deaths, or considerably less than half his mortality. His result being 1 death in every $69\frac{3}{4}$ cases, and mine 1 death in every 146 cases (see Table No. 1). It will be observed too that the Surgeon's mortality, owing to his freer use of Quinine, is very much smaller than the previous average for Fever, in the same Regimental Hospital, for previous years, which had always averaged for Fever 1 death in every 30 cases, for 20 years previously (see Table No. 4). My results in the ward of the General Hospital were compared with this more just standard, and resulted in 1 death in 129 Fever cases, the average for 20 previous years, viz., from 1830 to 1849, the date of my experiment having been 1 death in 11·256 Fever cases. After the close of the experiment, I was appointed to the charge of a European Regiment, and went with it on

service, during the war in Burmah, and afterwards through all the exposure in the mutiny and siege of Delhi, and the result has been during 9 years, under a decidedly improved method of administering Quinine, a further reduction of the mortality by Fever; for, of 6,982 cases of Fever treated during 9 years, in the 1st and 2nd Bengal European Regiments, 33 have died, or 1 death in every 211.193 cases, which is less than half per cent., or, in other words, the mortality by the dreaded Bengal Fever is almost reduced to nothing. Of the Tables from which these figures are taken, and which are given in the Appendix, some are taken from the Medical Board's Report to Government, 1849-50, of which a copy was supplied to every Medical Officer in the Indian Army, and the remainder from the Regimental Records, in the Office of the Principal Inspector General, kindly supplied to me by his Secretary, Dr. Macpherson. The results also of this experiment must be viewed in a very different light from those of private practice, for the experiment was performed in the public wards of two large Hospitals, under the direct superintendence of Dr. Nicholson, Surgeon of the General Hospital, Sir James Thomson, under whose particular superintendence the experiment was placed, and the Surgeon of Her Majesty's 70th. The wards also were constantly visited by other Medical Officers in Calcutta, who were much interested in the result of my trial.

But, lest one year might be considered too short a trial, I have continued the experiment through nine more years, with a European Regiment in Burmah and the siege of Delhi. I have, in fact, been in persistent trial of my treatment for Fever since 1843, viz., for 20 years.

I will now describe fully my treatment in detail, and the theoretical conclusions I have deduced from it.

My arrangements for the experiment on Fever were to give Quinine a full trial as an antidote. I gave it, therefore, irrespective of all variations of symptoms, except that the more violent and more dangerous these symptoms appeared, the larger, earlier, and more frequent was the dose of Quinine given. My Assistant was directed to mix a large bottle full of Quinine, rubbed up in plain thick mucilage, a scruple to the ounce. I would not give it in its more soluble form with diluted Sulphuric Acid, because I feared that, in the frequent and large doses I proposed giving, the quantity of Sulphuric

Acid required to dissolve the Quinine being considerable, it might injure the stomach, and besides, being an experiment which I wished to be simple, and free from all objection, I preferred giving the Quinine pure. One dose of a purgative, viz., a scruple of Compound Jalap Powder was allowed to be given in some cases, but latterly, especially during the rains, the purgative was very rarely given. All the worst cases, in fact, had no purgative, for I was too anxious to give as much Quinine as I could, and as early as possible, and was afraid of losing time, by nauseating the patient with other medicine. Moreover, I believe that purgatives delay and prevent the action of Quinine, and remove it too soon from the system. Sydenham and many other practitioners have observed, "that we are in danger of bringing back Intermittent Fevers, if we employ purgative medicine too soon after we have stopped them by Bark," and we have the same observation in Dr Haen—Cullen, vol. 1, p. 642."

The patients came in at uncertain hours, and I could not be always in the wards. I therefore gave a standing order to my Assistants, that the moment a patient was admitted, he was to be given a scruple of Quinine. I saw him always myself a short time afterwards, and gave him another scruple or half a drachm according to the urgency of the symptoms. In visiting the patients, an Assistant accompanied me, carrying the large bottle of Quinine and Mucilage described above, and a two-ounce measure glass, and each patient had an ounce from the bottle, or a scruple of Quinine given, and swallowed before me. Ordinary cases took this dose three times a day, but if the Fever was dangerous, I gave ʒiiss. of the solution, or half a drachm of Quinine at once, and a scruple every 3 or 4 hours afterwards, according as I found the stomach would bear it without vomiting. It was very rarely necessary to continue this treatment beyond the second day, after admission. The patient was almost invariably convalescent on the third day, when five grains doses three or four times a day, for about two days more, with good food, completed his cure.

Throughout the treatment, if the patient had had his Fever long, and thus being without appetite, had taken no food for some days, I gave him moderate quantities of fluid nourishment without delay, as arrowroot and milk, and soup of moderate strength, and if he were a man of free or especially intemperate habits, I allowed him, if he had been ill a few days

and without his usual stimulant, a small quantity of brandy or wine.

I may safely remark that, in the Annals of Medicine, such an experiment as the above, in simplicity and decisive results has never before been recorded. If the Table No. 3 of the mortality in the General Non-Military Hospital, be compared with the Military (Table No. 4) wards of the same range of building, the difference will be seen to be very great. In the Non-Military, during 20 previous years, there was one death in every $11\frac{1}{4}$ admissions for Fever, whereas in the Military the mortality was only 1 in 30. And the cause of this difference will be at once perceived from the very different description of cases admitted in each. The Military patients are simply the Soldiers of the Garrison of Fort William, who are sent to Hospital on the first appearance of sickness, whereas the cases admitted into the Non-Military wards are from the unemployed sailors wandering about Calcutta, and in fact any European who is sick, and has no other home to shelter him. Sailors and others are thus, after many days' debauch, picked up from the roads of Calcutta in an insensible state, and sent to the Hospital. They are, therefore, the most hopeless class of cases that can be treated in the deadly climate of Bengal, and their treatment, having been so long delayed, becomes most difficult. The large mortality, 1 in every 11 cases of admission for Fever, for the 20 years before my experiment, proves how severe the cases must have been which I treated there. The mortality also for Dysentery, 1 in every $4\frac{1}{2}$ cases treated, during the same period, (see the Table No. 3) is another instance of the enormous mortality which had been habitual from the above cause in the Non-Military General Hospital.

My treatment, therefore, comprised the young robust Soldier treated immediately after his sickness appeared, and the debauched, drunken outcast, from the streets and ditches round Calcutta, where they were frequently found by the Police, after they had been lying there all night. And it must be carefully remarked, that it was in these worst possible cases where the success of large immediate doses of Quinine was most wonderfully conspicuous, by reducing the mortality from 1 death in every 11 cases, to 1 death in 129.

I will now, having explained what the experiment was, state the deductions which I think must be drawn from the result.

In the first place, no blood was drawn, either by lancet or leeches.

Bleeding, therefore, is not necessary, and the disease is not inflammatory. No opium, no purgatives to bring away bad secretions, no drug of any kind except Quinine, is required for the successful treatment of Malarious Fever.

Quinine also may be given in the largest doses, whether there are head symptoms, delirium, coma, or pain in the liver. Whether it be in the hot stage or the cold, Quinine is not only safe for all forms of Malarious Fever, but its certain cure. And, in cases where there is danger to life, the earlier and the larger the doses of Quinine which can be given to the patient, the better. Vomiting and full cinchonism were my only checks for continuing it in full doses.

I mean by cinchonism, the peculiar head symptoms, which large doses of Quinine produce, and when they were fully obtained, I considered my patient safe, and that only moderate doses were then required. I always waited too, till the cinchonism had lessened, before I continued, even small 4 to 5-grain doses, three or four times a day.

Quinine, therefore, may with reason be pronounced to be a direct antidote to the poison of Malaria, and not simply an antiperiodic, and adapted only to stop periodicity, for it always cured equally well those Fevers in which there were no periods, but which continued without the slightest remission during the 24 hours.

Also I will explain a deduction from my experiment, which I had long meditated before I went to Calcutta.

It had constantly struck me, that all the Fevers I met with in India were caused by Malaria. All infectious Fevers, Typhus and Typhoid, are unknown in the plains of India, among the European part of the population. I have met with only two cases of Typhoid Fever, with ulceration of Peyer's glands, during my practice in India, and they were both recent arrivals, from on board ship, and these cases I saw after my Calcutta experiment. I therefore had come to the conclusion, that with rare exceptions, all Fevers in India, whether called ardent, continued, continued congestive, remittent, or intermittent, were all caused by one only cause, viz., the blood poison malaria. It was my conviction of this truth, which justified my indiscriminate and determined dosing with Quinine every Fever patient admitted into my

wards, and I was guided a good deal in my plans, by having read, and thought over, the following passage from Dr. Holland's Notes and Reflections, page 150: —

“ And as Bark by curing, associates together many intermittent affections, under the presumption of a common cause, so may colchicum furnish similar inference, regarding affections, seemingly remote in situation and symptoms. This mode of inference, hitherto very limited, may hereafter be extended much further. It is one of singular value, as closely connecting the treatment with the theory of disease, and rendering them mutually corrective of each other. It at once enlarges and simplifies our views, in every part of Pathology; each fact well ascertained, is fruitful of results far beyond those which directly appear to the view.”

Again, page 348, he enumerates these as proofs of a disease having a common origin with Ague, viz., Malaria.

“ 1st.—By their intermissions.

“ 2nd.—By their succession to, or alternation with, Intermitting Fever.

“ 3rd.—By the influence of the same specific remedy, in relieving or removing such disorders.”

He says the application of these proofs to particular cases might be largely made.

Again, page 354—“ The influence of certain medicines, and particularly of Bark, in curing even the most anomalous varieties of these intermittent disorders, is a fact of great interest. Like the use of mercury in obscure syphilitic affections, or colchicum in the most irregular forms of gout, it enables us to denote and class together, symptoms apparently the most remote in kind, but which presumably could not thus be relieved, unless depending on some common cause. We have, therefore, in the specific nature of the remedy, a sort of practical test of the character of the disease, often of great importance to the consistency and success of our treatment, and related through the principle of the enquiry to some of the most interesting questions in all Pathology. This it is, as was noticed when speaking elsewhere of colchicum, which gives peculiar value to all which illustrates the action of these remedies. They are the interpreters of facts, far beyond their momentary effects,

“and of connections between morbid states, which are in no way so definitely made known to us.”

The success of this method is acknowledged, in establishing the gouty nature of many diseases of the joints, eye, heart, &c., by colchicum. By mercury, in testing the syphilitic nature of many diseases of the skin, iritis, &c.; and by Quinine and Arsenic, in proving many neuralgias, and other very many chronic diseases, described by Macculloch to arise from Malaria. But I believe that I am the first who ever thought of applying the principle, connected as it is with some of the most interesting questions in all Pathology, to acute diseases, viz., all the Fevers of India. And let me remark, that colchicum and mercury, being purgative and acting freely on all the secretions, may act thus beneficially on all these diseases, apart from any specific effect, but this cannot be said of Quinine, which acts neither on the bowels or any of the secretions. This reasoning, therefore, applied to Quinine may be received with greater certainty than to the others, and yet remarkable and valuable results have been obtained from experiments on the latter. It may therefore be received with the greatest confidence that the fact that 421 Fever cases, indiscriminately admitted, during all the seasons of the year, were, I may say, saturated within two days after their admission, with Quinine, with only three deaths in two Hospitals in Calcutta, where the usual mortality had been for twenty years previously, 1 death in 11 cases, and 1 death in 30, proves beyond dispute to my mind that all these Fevers had a malarious origin. For mark, it was not only that Quinine was given with impunity, that is, that it did not increase the average mortality, but it almost completely stopped a mortality which had been, before its use, enormous.

A faithful history also of my Calcutta experiment obliges me here, though I do it with great regret, to notice an unfortunate accident which happened to me during its course: and which has often since been quoted to my discredit.

In the spring of the year, a violent epidemic of small-pox, and a bad form of measles, broke out in Calcutta, and many of these cases, in their primary febrile stage, were sent to my ward as acute Fever cases. My rule was invariable to my assistants, to give every patient a scruple dose of Quinine on admission, and these cases, having often violent febrile symp-

toms, had the large dose given them; and even after my own inspection, the Quinine was frequently repeated, for I could not, I confess, till the eruption appeared, always distinguish the Fever stage of small-pox or measles, from any other Fever. The mortality among these cases was very large, though of course the moment I detected what their disease really was, I asked Sir James Thomson to inspect them, and have them removed to the other wards of the Hospital. Such unfortunate accidents, in an indiscriminate experiment like mine, was unavoidable, and my only consolation since has been, that on the whole I saved life largely, though in causing a few deaths, I was unfortunate. And if this misfortune, which has often been brought forward against my experiment, by those who were on the spot and witnessed the sad result, be considered calmly and reasoned on, it shows more clearly than any number of other facts could possibly have done, what enormous mortality I must have had among my patients, if the other Fevers which I treated had not been all malarious. It may be considered, therefore, as demonstrated by my Calcutta trial, without referring to my 6,982 Fever cases since treated, in the same way with Quinine, in my Regiments during nine years, in every part of India and Burmah, that $\frac{999}{1,000}$ of the Fevers in India, have a malarious origin. And this I must consider to be a most valuable discovery, not even a trace of which can be found in any of the Literature of Tropical disease to the present day. It was this point, also, which gave me more anxiety than any other before I went to Calcutta. For though I had tried the Quinine treatment, with uniform success in the Fevers of Upper India, yet the number of varieties of Fever, described as having different causes and symptoms, by medical writers of high repute in Calcutta, Sun Fever, ardent continued Fever of the hot weather, congestive Fever of the cold, Inflammatory* forms of continued Fever, &c., made me very anxious lest I might not cause large mortality, by using an indiscriminating Quinine treatment.

In fact, a Presidency Surgeon, in the most extensive practice in Calcutta, warned me on my arrival, in the presence of Sir James Thomson, that if I gave Quinine to Fever cases with head symptoms, I would certainly kill

* Annesley, vol. 2, p. 511

them. I was startled by this assertion, and at first timid, but I went on, trusting in my reason, for I had carefully examined the reported cases of these varieties of Fever, and had seen so many of them changing one into the other, that I felt certain that Dr. Holland's second axiom might be applied. "By their succession to, or alternation with, Intermitting Fever," and that whatever was the cause of ague, was the cause of them all. Thus, Dr. Stokes says, that bleeding, according to Dr. Mackintosh's system, has a great tendency to convert ague into continued Fever, and vice versa. Many of Dr. Twining's cases of continued Fever, in their convalescence, terminated in ague; Remittent Fevers constantly ending so. I cannot, of course, quote these cases which decided my opinion, and which would form a volume. I merely attempt to describe of what kind my reasoning was. I had seen also one case, and I have often witnessed them since, of what Dr. Haspel in Algeria called Algid Fever, where my patient remained for a full 36 hours as cold and blue as in the collapse of cholera, and under large doses of Quinine, when he was at length fully cinchonised, the first symptom as I may call it, of returning life, after the cinchonism had subsided, was a violent paroxysm of burning Fever, which a few more doses of Quinine cured. I made an analysis, too, of a large number of the different varieties of fever, recorded in the old Diaries of the General Hospital, and the result would convince the most captious, that they were all one and the same disease, frequently interchanging its symptoms.

I will quote a passage from the Medical Board's Report, which they have selected from my monthly reports to them, and thus authenticated it with their approval, page 21. I write, "when I arrived in Calcutta, it is known to the Board, "through my first report, how anxious I was to ascertain "what varieties of Fever were met with in Calcutta. I felt "that the success of my experiment in Fever depended on "this knowledge, for I could not but anticipate great "mortality from my large doses of Quinine, if there were met "with here, many varieties of Fever not malarious. This "accident did occur, in some cases of small-pox and "measles, which were sent to my ward as Fever, in their "early stages. It may be conceived, therefore, how carefully I examined every author I could meet with. These "authors told me nothing, none of them even attempted

“ to classify Fevers by their causes. All that they give,
 “ is a few Fevers, named after some prominent variety in their
 “ symptoms, such as intermittent, remittent, continued, con-
 “ gestive, inflammatory congestive, ardent, &c. No author
 “ attempts to prove their causes, or, if they do, it is in a very
 “ obscure way. Twining, for instance, in a note, attri-
 “ butes Fevers to the irritation from inflammation and con-
 “ gestion of internal organs. Johnson ascribes them to sudden
 “ changes of temperature, and congestions of the liver, others
 “ to sudden cold, and so on. Now I claim as my own, that
 “ I have clearly ascertained this point, for out of 421 cases of
 “ Fever which I have treated here, during all the seasons of
 “ the year, many of these varieties must have certainly occur-
 “ red, and yet they have all been cured by Quinine, not cured
 “ by Quinine mixed with calomel and purgatives, but by
 “ Quinine alone, and given in such enormous doses, that it
 “ must have caused death, had the liver, checked perspiration,
 “ or any other such cause been the origin of the Fever. All
 “ these Fevers have, moreover, been cured at once; for I think
 “ I may safely challenge a comparison of my average time
 “ of treatment, with any other returns for Calcutta Fevers.
 “ My average period, I think, will be found much* smaller, and
 “ the improvement has followed instantly on giving the remedy.
 “ I have had no cases so plentifully found in the diaries here,
 “ to-day improved, to-morrow worse, and all the phases of a
 “ fierce struggle between a strong constitution and the disease.
 “ I have really had no cases I could describe, except that they
 “ came in bad, were next day better, and on the 3rd or 4th
 “ day convalescent. I have had no black tongues, no nights
 “ of delirium, &c., &c., which are the substance of our record-
 “ ed cases of Fever. The disease is checked by Quinine in
 “ 24 hours, and finally removed in 48, weakness only re-
 “ maining to detain the patient in Hospital. No leeching,
 “ no blisters, no foul evacuations by stool. It is one rapid
 “ progress, from often the last extremity, to permanent
 “ health.”

I reason, therefore, as Dr. Holland reasons, “ that if the
 “ same antidote cures all the 421 Fevers which I have treated
 “ this year, viz., continued, congestive, &c., they must have all

* The Board say in a note, “ This is borne out by the comparison of the re-
 “ sults as shown by other returns received at the same time.

“had one and the same cause, viz., Malaria.” Naturam morborum remedia ostendunt, and, as Dr. Holland says, “The treatment and theory of disease may be rendered by this principle mutually corrective of each other, viz., the cause of the disease ascertained from the effects of the remedy, and the right remedy ascertained from the cause.”

Dr. Holland lives in a society tainted with gout, we in India practise among patients who are universally affected with malaria. There is this difference, however, between us, and it is much to my advantage. Colchicum purges and is a diuretic, and *may* cure many diseases, therefore, such as repletion and inflammation, &c., by its general antiphlogistic action. But with Quinine it is not so, and therefore it is by far the most important test. Again, Dr. Holland had only one test, I have two,* viz., Quinine and Arsenic, and the latter I know from experience to be in many cases a still more powerful antidote than Quinine; and it also produces no evacuation, for it ought never to be given in such doses. There can be no doubt, therefore, of the truth of the principle, and the ample means we have of accomplishing the whole design, and let it be well considered how great that design is. It is a classification of all malarious diseases under their true cause, and these diseases are the scourges of mankind. It gives us the certainty of treating them by direct antidotes, instead of by the common antiphlogistic system. It will give us all the varieties of symptoms, caused by the malarious poison, with such minute exactness, as to excite a strong hope of ascertaining what that poison is,—certainly of at once detecting the error, if a wrong one be proposed.

* Mercury might also be added to these two tests, though its very general curative virtues in other diseases make it less certain than the other two. I would myself, however, strongly suspect a Fever to be malarious, if salivation at once stopped it, for I know of no other form of Fever where it would have the same immediate effect. Mercury also is used as a test for venereal diseases, for most of us, I think, would pronounce, with tolerable certainty, a doubtful skin eruption, &c., to be syphilitic, if mercury cured it. Arsenic has not yet been used as a cure for the acute forms of Malarious Fever, because we do not want it, having another safer remedy. In some acute Fevers, however, for which I have tried arsenic among Natives, its curative results were very remarkable. I gave it in 15 to 20 minims of Fowler's solution, four times a day, till it produced nausea. There is no reason, moreover, why we should not try Fevers with all the three tests, and if they all cured the same form of Fever, our results would be certain.

The importance, however, of this reasoning will more fully appear, when, as I propose to do, I discuss in my next paper the difference between malarious and common dysentery, and the very marked change of treatment which they individually require. I would also, in passing, throw out a suggestion to our English practitioners, to test a few of their spring and summer Fevers by this method. I have a very vivid recollection of the miserable feelings which I endured from a spring Fever myself, when a boy at school, which I know now from the symptoms I had, to have been a Malarious Fever, and which might have been cured in two or three days, by a few mild doses of Quinine, but which I endured, and with great suffering, under salines and purgatives, for nearly four weeks. It certainly also would be a great advantage, if we could separate more distinctly all these Fevers, from the great class caused by infectious animal poisons, typhus, typhoid, &c. Our treatment of Fevers in England ought to be directed more to an attempt to discover other antidotes, and not to rest so idly content with the mere treatment of symptoms, which *can* lead to no discoveries or great improvements. Any old woman can, and always does treat her patients by symptoms; and if the head is hot, apply cold, if the feet be cold, put them in hot water, give food, and so on. In India our Fevers are so violent and rapid that such treatment could not satisfy any one, and being thus compelled to try powerful remedies with activity, at last through many vicissitudes, we have reached our harbour.

Dr. Todd did attempt the antidotal treatment of typhus with alcohol. Many failures must be expected, but in the end no, doubt, chemistry and experience will give us antidotes for other Fevers, as they have already done for the great class caused by malaria, and the less we acquiesce in the idle system of allowing Fevers to run their course, the sooner we shall arrive at this desired result.

I had another strong reason for examining carefully the old diaries of the General Hospital, still existing since 1797, viz., to decide whether any proof of inflammation, either in the head or abdomen, could be found in the *post mortem* descriptions, recorded after death from Fever, and I made an analysis of every fatal case, about 700,* and

* See the Board's report, p. 14.

reported the result to the Medical Board; and in not one of them was there described any result of inflammation from Fever, whereas the *post mortem* examinations for dysentery proved that acute inflammation was, in it, most common—viz., effusion of lymph, gluing the intestines together, and covering their surfaces; also effusion of lymph into the substance of the colon, causing thickened solid enlargement of it.

From Fever the only results observed were, venous vascularity of the membranes of the brain, and effusion of clear serum into the ventricles; but never any trace of lymph on its surface. In the abdomen, also, nothing was seen, but extreme venous congestion, the mesenteric veins enormously enlarged, and the liver and spleen gorged with venous blood, greatly enlarged, and when cut into, appearing so softened and disintegrated, that they often fell to pieces in the hand. Annesley strongly advises, that in examining the abdomen of Fever patients during life, the abdomen, liver, and spleen should be very gently pressed on, lest on account of this softening, even slight pressure might cause fatal injury.

In fine, all the *post mortem* appearances from Fever were extreme venous congestion of the abdomen and brain, and passive effusion of serum into the brain, substance of the liver and spleen, thereby causing the softening of its texture.

In fact, why may not the reasoning of Dr. James Johnson, on the non-inflammatory nature of Intermittent Fever, be applied also to Remittent, and thus to all those Fevers which interchange symptoms with them.

What kind of inflammation must that be, which explodes, as it were, the moment the clock strikes a particular hour, and vanishes the moment it strikes another hour, and this for days together? What kind of inflammation is that, which every day terminates in perspiration from head to foot, and yet is renewed after an interval of 24 hours, with symptoms as before, and so on. Do we see real and unequivocal inflammations pursue such a course? Will bleeding also in true inflammations, if used at one hour of the day, relieve the symptoms; and two or three hours after, cause death in the same day, and this every day, sometimes for a week or ten days? Are not the changes in Remittent Fever, from the accession every night to the remission in the morning, quite

sufficient to warrant the application of this reasoning to it, as well as to ague? And yet we read in descriptions of Remittent Fever in 1861.*—"It must be held in recollection that "what are termed congestions, sub-acute and acute inflammations, tend, firstly, to destroy life speedily, and, secondly, in "their remoter results, they found organic diseases, which "have eventually the same fatal effects."

All the phenomena of fever may indeed be traced to one simple fact. That the blood is in some way poisoned, circulates with difficulty through the capillaries, and stagnates particularly in the abdominal viscera, and the portal system of veins, which feels less of the assisting action of the heart, and is almost entirely dependent for its circulation on the attraction between the blood and the tissues which its capillaries supply, to 'carry on the vital fluid. The blood seems, from the effect of the poison, to have lost this attraction, and in part also its vitality. It is impossible, however, that we can describe, or perhaps even conceive, the process of any vital action, and particularly one like that in the capillaries, where secretion is effected, and the conversion of blood into living flesh is performed, and it may be that the capillaries reject the poisoned blood, which comes to them to be digested and vitalised, as the stomach, acting under the regulation of the same system of organic nerves, rejects improper food. This would bring us back to the old theory of Cullen, spasm of the extreme vessels. But, however explained, the fact remains, that the blood does not pass through the capillaries readily, and is constantly stagnating, and nature also, is constantly striving to drive it on by paroxysms.

In some cases, nature keeps the upper hand till the patient is cured, and we have then continued Fever, which is always short and without danger, but in others, nature becomes at intervals exhausted with her struggles, and we have remissions and Remittent Fever. In the last, the poison paralyses nature in toto, congestion has the supremacy, the circulation nearly ceases, and as, in a very similar case, the congestion of cholera, the patient is icy cold, for sometimes 48 hours' continuance, and frequently dies without any re-action at all. This is the icy Agid Fever of Algiers, and is not uncommon in Burmah and Bengal.

* Dr. Martin, p. 319, 1861.

The re-action which we notice in Remittent Fever, is no more inflammatory, or requiring blood-letting, than the excitement described by Marshall Hall, as following excessive blood-letting, and which is merely an effort of nature to overcome the stagnation of the circulation, from the weakened action of the heart, immediately following the depletion. It has been shown in a case above, that bleeding may be carried to the extent of blanching the rest of the body, and destroying life, and yet the malarious congestion remain, and the spleen be found gorged with blood. Also Dr. Johnson, who practised bleeding so freely, confesses that you may open a sluice, and yet not relieve the congestions.

That this extreme engorgement and distension of the veins of the liver and spleen with blood may result in injury to the texture, and by effusion of the fluid portions of the blood into its cellular structure, cause injury to the secreting portion, is certain: and effusion into the lobular cellular tissue, especially in chronic cases, by its pressure, causes absorption of the secreting cells of the liver, so that too little bile is secreted, and the blood becomes impregnated with it, giving a yellow color to the skin, and a bilious smell to the perspiration. In the brain, too, congestion constantly causes death from coma, which is the most common form of death in Malarious Fever; and yet so little tendency is there to true inflammatory consolidation and hepatisation, that we find cases of Fever, which have had symptoms of most extreme congestion, recover rapidly in a few days without a trace during the convalescence, of any injury to either liver, spleen, or brain, which show no symptoms of injury the moment the congestions and paroxysms are stopped. Were there any real injury of the textures sustained by these organs, we would have full intimation of it after Fever, as we have after inflammation of the lung, or kidney. The restoration to health would not be so immediate in Fever as it is, especially under the Quinine treatment, where weakening depletions are not used.

In Dysentery, where there is acute inflammatory action, how tedious is convalescence and what care does it require before the colon, injured in its textures by the inflammation, recovers its normal functions.

Also, in the chronic forms of malarious congestion, described so well by Dr. Martin, case page 757, the liver may

be enormously enlarged, extending down to the umbilicus, and remain thus congested for three months, and yet on this patient, dying of another disease, the liver was found quite healthy.

Several other cases are recorded by Dr. Martin, of patients where the liver and spleen continued greatly enlarged for some months, and yet this enlargement was reduced, and the patients quite recovered their health. In one case, page 665, the waist was reduced in size 7 inches; again a patient, page 772, "whose liver was greatly enlarged, April 6th, extending down to near the umbilicus, hard and very painful on pressure." On his death, October 11th, Dr. Martin says—"separated from his general ill habit, there was no structural disease to cause death, or indeed, under more favourable circumstances, to preclude the recovery of health."* The enormous spleens also arising from ague, which subside by change of climate or taking Iron, are of constant occurrence. The spleen, in these cases, seems like a huge sponge, which fills and empties itself, without sustaining any important injury to its texture.

The chronic results of malarious disease point plainly to the fact, that the tendency of the poison is to devitalise the blood, and to depress all the powers of life. Frequent attacks of Fever, or even long residence alone in a malarious climate, produce anemia. A yellow pasty color of the skin, emaciation, weak action of the heart, and intermitting pulse, dry furfuraceous state of the skin, and scorbutic state of the gums. The hair becomes a half-starved scanty down, which grows slowly, and the nails even cease to form; there is inability to take exercise, and premature old age.

Dr. Martin, page 657, speaking of an originally plethoric Officer, aged 32, who was treated for Remittent Fever, with bleeding, leeches, calomel to salivation, with a continuous course of powerful purgatives, the diet consisting of sago and arrowroot, says:—"He is now in a state of complete anemia, with a pale bloated lemon-coloured complexion, and mossy dry scanty hair; the abdomen is doughy and inelastic, the skin dry, harsh, and constricted," and he remarks, "this is an example of simple uncomplicated anemia, resulting from

* The case of Captain F. mentioned by Dr. Martin, p. 372, is no proof of inflammation in Fever, for he died of inflammatory dropsy in 1852, having had Intermittent Fever in 1837, without any intermediate relapse. The lymph, found 15 years afterwards, was quite recent.

“Intermittent and Remittent Fever, and their *necessary* treatment by blood-letting, mercury, purgatives, and low diet. “The patient had been under able and experienced medical treatment, but the malarious influences proved too strong to “be resisted, even by his strong European constitution.”

Again, Dr. Martin observes:—“The very treatment *necessary* to the cure of acute (malarious) disease, especially blood-letting, mercury, and drastic purgatives, along with the “most rigid abstinence, tends powerfully, even in young “persons, to produce a deficiency of red particles, while the “same course of treatment in persons of more advanced age, “along with their more protracted exposure to malarious “influences, are sure to diminish the old globules, and prevent “the formation of new—the debility, in fact, of anemia.”

In Burmah, at Rangoon, where I witnessed malaria in its most intense forms, I had young healthy patients, so poisoned that their blood became as much decomposed, as by the poison of a snake bite; and Hæmorrhage took place from the gums and nostrils: and, in one man with an open Bubo, there was uncontrollable dark venous bleeding. These cases were of frequent occurrence, and the face and lips had a blue venous tinge, and the surface was unusually cool. In what is called Hæmorrhagic Dysentery, described well by Dr. Raleigh in Calcutta, and of which I had numerous cases in Burmah: during the extreme congestion of the portal system in this algid Fever, (for, in reality, it is that disease, and not Dysentery,) the blood at length oozes from the gorged mesentery, through the coats of the bowel, and the patient dies, passing pints of pure dark blood, which will not coagulate, and his skin is like ice to the touch.

Dr. Twining says, “The altered appearance of the blood, in “many of these Fevers, which arise from exposure to the “malaria of the jungles, is very remarkable, and merits careful investigation. Connected with this morbid condition of “the blood, there appears a disorder of the vascular system “which favours prolonged and profuse oozing of blood from “leech bites.” Precisely the same oozing of blood from leech “bites, has been often observed after the bite of a snake.”

The blood, in fact, is devitalised, putrid, as the older physicians style it, and there is not one trace of the possibility of inflammatory action, but of its extreme opposite, viz., partial death from a deadly depressing poison.

I have strongly in my memory as I write this, the case of an officer on his first arrival in Rangoon. His Regiment was to move up the Irrawaddy, he was known to dread the climate of Burmah and its discomforts, and was supposed to wish to get away from his Regiment. I was told that there was nothing apparently the matter with him, for he had neither Fever or Dysentery, and was a stout, tall, athletic young man. I found him lying in bed quite sensible, but his face and lips were blue, his breath was cool, and his hand felt like a dead man's. I have, in some cases, distinctly smelt an earthy smell, like that from a corpse. He lingered some days in the same cold state, at last the blood found vent, from his bowels, and he died with profuse Hæmorrhage. The *post mortem* appearances in such a case as this, are well illustrated by a case of Dr. Macpherson, in the General Hospital, "E. Rogerson, "September 1st, 1849, Dysentery, died 15th, of the putrid and "passive form of the same disease." Dr. Macpherson remarks on the *post mortem*—"There was a general oozing of blood "from the mucous surface throughout to the anus. A large "quantity of blood was found in the rectum. There was no "general ulceration of the intestine." He again writes,— "This was a case of simple Dysentery, which was yielding to "treatment, when Fever supervened, after which the Hæmorrhage tendency developed itself." There are many other similar cases in the General Hospital Records, and the notice of a deep purple color of the colon after death is very common, showing the extreme venous congestion of the colon and the portal system. I cured several of these cases in the General Hospital, and it was to them I gave Quinine in the largest quantity. In some cases the amaurosis and deafness, resulting from the Quinine, continued for two days. But both senses were, eventually, quite restored.

As instances of Terai Fever, I may mention the Residency Surgeon of Nepaul. He passed through the Terai incautiously and died on his arrival at Benares, in a state of blue collapse, very similar to Cholera; and the worst case I ever saw of Terai Fever, was, Assistant Surgeon W——, a strong active man, and celebrated for the fatigues he could undergo in shooting. He was delayed for one night in the Terai, at the foot of the Nynce Tal Hills, and on his arrival at Simla, had all the symptoms of delirium tremens, so much so, that many thought, though he was a strict water drinker, that he was

suffering from intemperance. This strong healthy young man, from a few hours' exposure, became blue and livid; he staggered when he attempted to stand; his hand and tongue were tremulous; his eye dull, and though he moved about, he seemed unconscious of what he was doing, and he died after some days, without treatment, of coma.

Simple stagnation of the blood in the veins of the portal system, must rapidly render it loaded with effete matter, and very impure, without considering the addition of a poison, and Mr. Branshy Cooper says that, in a similar condition of a varicose vein of the leg, the discharge of such blood by puncture of the diseased vessels, does not produce any constitutional effect. The blood contained within the varicose veins, being to a certain extent thrown out of the general mass of the circulation, and retained in a half stagnant state within them. The state of the circulation of the abdomen in the congestion of Fever, is like a small stream running through a large pool on one side of it; there is a current on this side, while in the remote parts of the pool, the water is stagnant; or rather it is like a large aneurism, where the blood passes in its usual course on one side, while the other is filled with clots and devitalised blood. It might be good practice, if we could relieve the oppressed liver, by emptying it, like a varicose vein, of its stagnant blood; but when this blood is taken from the general circulation, where it is still healthy, it appears indeed to be depriving the patient of the little good blood he has left. For it must be remembered, that the whole of this stagnant and devitalised blood has already been abstracted from the normal amount of the healthy circulation, being in itself, in its effect on the system, a huge blood-letting, or at least a large abstraction of vital fluid.

This stagnation of the blood in congestion of the spleen, has been long known to produce anemia, and doubtless it does so also in a more extreme degree, in congestion of the liver.

Dr. Martin observes, page 662—"The obstructed and depraved state of the blood, and consequent depravation of all the secretions and excretions, and of assimilation also, must not be overlooked. We sometimes find the heart impaired in its functions, through long and often continued engorgements of the great venous trunks during the cold or congestive stage of Remittent and Intermittent Fevers, and from the violence of the subsequent bounding re-actions.

“Through the operation of these causes, associated as they often are, with an anemic condition of the system, the heart is occasionally found to be flaccid in its texture, and dilated in its cavities, while its action is feeble and irregular. In every form of Malarious Fever, but more especially in the tremendous remittents of hot climates, it is manifest that the sensibility and energy of the entire nervous system are blunted,* and enfeebled, and that the muscular system, voluntary and involuntary, is relaxed and enfeebled also in consequence both of the lesion of the nervous system, and of the altered condition of the blood, so common to tropical fevers, and as resulting *from their treatment.*”

It must be considered, therefore, a very great improvement, that, considering the essentially weakening anemic results of the poison of malaria, when acting alone, we need not now increase these tendencies so largely, as has hitherto been thought necessary, by our treatment with blood-letting, copious purging, and starvation. In my Calcutta experiment, none of these were used, and good diet was given throughout, as much as the stomach would bear; and the Quinine early and freely given, quickly removed the congestions, and consequently prevented the resulting depravation of the blood. The consequence was that there was no tedious convalescence, but the patients rapidly recovered, and seldom remained longer than a week in Hospital. And moreover, I have never, yet seen, under this Quinine treatment, as so often results with other methods of treatment, that the acute form terminates in the chronic, that is, remittent ends in intermittent, with all its anemic tendencies. But I will quote what others have written.

* It is probable that the constantly recurring congestions of Indians returned to a cold climate, is partly owing to the injury, or partial paralysis which the nerves which regulate the circulation of the abdominal viscera have sustained. This circulation being thus always feeble, the slightest accident which increases the impurity of the blood, especially checked exhalation from the skin, will bring on distressing congestion. It seems as if these nerves were so weakened that they could not act on the vessels with sufficient power to circulate the blood onwards, and thus, in some cases, a state of chronic congestion results, with probably a varicose state of the veins, from repeated overdistension. This is more reasonable than to suppose, to account for this fact, that in a cold climate, the malarious poison still continues active in the blood, during so many years.

Dr. Ewart,* page 369, says: "The Medical Board's most favorable Report in 1851, on Hare's experimental trial at the Calcutta General Hospital, as also under the Surgeon of Her Majesty's Regiment in Fort William, 1849-50, gave the final death-blow to spoliative treatment by depletion, as completely as that of spoliation by mercurialization, had been sealed many years before. And here I feel bound to express my humble tribute of esteem for, and admiration of Mr. Hare, for giving the culminating death-blow to those relics of spoliative treatment which existed in too great profusion, at the time when he first attracted notice, by the publication of his views for braving the active or passive opposition of many of his professional brethren, many of them being his seniors, in a strictly seniority service. By undertaking an experiment, the results of which established the fact that Malarious Fevers, whatever be their complications, were curable by generous doses of Quinine, for affording conclusive testimony that it is perfectly safe to give the drug during the paroxysms, for succeeding in inspiring the profession with the belief that it was the soundest practice to exhibit larger and therefore more effective doses, than it had been the custom of its members previously to do: for in short finally, and it is earnestly hoped for ever, sealing the doom of unnecessary spoliation of every kind and every form, and giving an irresistible impetus to the introduction of the present anteperiodic and conservative system which now reigns with undisputed supremacy, wherever the medical science of the West is called upon to oppose the progress of Miasmatic Fevers to the honor of a noble and humane profession, and to the incalculable and unspeakable benefit of mankind."

Lord Dalhousie, too, immediately he knew the favorable result of my experiment, and became aware from the Medical Board of the large increase of expenditure of Quinine it would cause in our three Presidencies, with his usual wise forethought, wrote to the Court of Directors to make some strong efforts to introduce the Cinchona tree to India, and a gentleman was sent by them to South America, and an application to the Dutch Governor of Java obtained also other plants, which

* Indian Annals, No. 14, 1861, Review of the treatment of Tropical disease by Dr. Ewart, Professor of Anatomy and Physiology, Medical College, Calcutta.

are now flourishing in large plantations in Ceylon and the Neilgherry Hills; and the Cinchona, the tree for healing the nations, is now firmly established in our soil. And to prove how very general the practice by Quinine has become, I give from Dr. Ewart's valuable History, a Table (No. 6) showing the rapidly increased expenditure of Quinine, from 1849-50, that is, before my experiment was finished, to 1859, after it. From this Table it will be seen that the expenditure of Quinine in the Bengal Presidency alone, has increased from 520 lbs. yearly to 1,656 lbs.

In reviewing also the results of the experiment, a necessary question arises—How does Quinine act? It was introduced to the world as a tonic, and it produces very remarkable head symptoms. So long, therefore, as the congestions of Fever were considered inflammatory, and Fever to require active depletion, we need scarcely wonder that it should have been so carefully excluded, especially when the Fever affected the head. And it was this prejudice in particular, which I especially went to Calcutta to disprove, for I knew that these latter cases were the most fatal, and therefore those requiring the largest and earliest doses of Quinine, and the cases with termination to the head were those whom, in my experiment, I especially attacked vigorously with my remedy, and it was soon evident to me, as it will be to any one who tries the large doses, that so far from exciting, Quinine produces the most deadly coldness of the skin, feeble pulse sometimes intermitting, complete deafness and amaurosis, evidently from its powerfully sedative, and almost paralysing action on the brain. The delirium it causes also, is not active, but a low muttering wandering. It is in fact to me quite certain, that no one had ever tried Quinine in acute Fever, except in small doses, when it does produce sometimes a peculiar excitement. But in large full doses, it is the most powerful and alarming sedative. I have, during my practice, often been frightened with my own results. I recollect one case, which made a strong impression upon me. It was in Burmah. A young Officer of my Regiment, Lieutenant M——, was sent in from an out-post, having had Fever for some days; and he was reported to me as having been delirious the night before, and in great danger. I saw him first about 3 P. M., and found him in the most hopeless state. The accession was just coming on, and I knew that he must die in the night,

if it was not checked. There was, therefore, no time for delay. I instantly gave him 3ss.; at 5 p. m. I gave him ℥i.; and at 7 p. m. ℥i. more; or 70 grains in 4 hours, and I sat down by his bed side to watch the result.

No paroxysm appeared, it was quenched by Quinine, as water does fire. His skin became deadly cold, his pulse fluttering with complete deafness and muttering delirium. He was very restless, complaining that he saw devils floating before his eyes. The pulse alarmed me greatly, and I gave him repeatedly a glass of Port Wine. In the course of about 4 hours, however, these dangerous symptoms subsided. He fell asleep, and the next day was employed, not in giving him medicine, but food. His Fever left him from that night, and he was soon well, and is now strong and robust, and an ornament to his profession.

Major S——, of the same Regiment, was almost a similar case, and I could enumerate numerous others. My Regiments on two occasions, in the Burmese war, and during the siege of Delhi, subscribed largely from their canteens, to purchase a private stock of Quinine, lest my public stores should run short, and to enable me to give them Quinine unsparingly.

No one, in fact, living, has given Quinine more largely than I have, and though I have as yet had no fatal result from it, I have often dreaded such misfortune, in cases where I felt justified in giving it in very large doses. And the dangerous symptoms have always been, not excitement, but deadly coldness, arrest of the pulse, and paralysis of the heart.

These, however, are the effects of *very* large doses.

I will now close my account of the Calcutta experiment. Its results were published to my service in India, by a copy of the Report on it being sent by the Medical Board to every Medical Officer. I was, at my own request, posted to a European Regiment, and I determined quietly to allow the seed I had sown to produce its fruit, and myself with the noble opportunity which the Surgeon of a European Regiment has, especially on service in Burmah, I resolved patiently, instead of perpetually writing, to wait my time, collect my results, and see what further experience would teach me. I have since, as the Table No. 5 shows, treated 6,982 Fever patients, with only 1 death in every $211\frac{1}{4}$ cases, and I have to describe but one change in my treatment, which, however,

I consider, a most valuable and important improvement. I have, in fact, gone backward, as it will be thought by some, in my antispoliative system, and I resort now, and I am certain in some cases with great advantage, to *Mercury*.

I have not, however, lost any of my old confidence in Quinine. I maintain still, that given early and in sufficient doses, it is a perfect antidote; and I would cheerfully go back to Calcutta, and try my experiment again with Quinine *only*, and a syringe with plain water, to wash out the bowels.

With water to wash out the bowels and apply to the head, and plenty of Quinine, I want no other medicine for the treatment of the worst forms of Tropical Fever. Leeches, bleeding, purgatives, emetics, blisters, are, if not useless, at any rate, unnecessary. You can cure all your patients without one of them. Why, then, do I now use Calomel? I will explain. Quinine, as shown above, when given in the large doses, which are occasionally required to save life in cases where the Fever has been long neglected, produces very dangerous and distressing symptoms. But I assert again, that I have never yet had a case where the results were fatal, nor do I know any case of mine in which even the patient complained of any lasting bad results from Quinine, though every Officer in the Regiment, as well as myself, must have seen them, had they occurred. For, in a European Regiment, every man's after-history is well known, and I was with them for nine years. Yet I can easily conceive of injury resulting from Quinine, where very large doses, as a drachm, are given at once. My rule was, not to exceed half a drachm given to commence with, and scruple doses at intervals of never less than 3 or 4 hours. With these doses the symptoms of Cinchonism will soon warn you to stop the medicine, before sufficient can have been given to poison.

However, besides this, any one who treats many bad forms of Fever will find himself sometimes puzzled how to act, when the patient vomits his medicine.

It may be written down as an axiom in all cases true—give plenty of Quinine to a case of Malarious Fever, and it will be cured. But this is not always so easy in practice as it appears on paper, for if you cannot give the Quinine, because the patient vomits it, what are you to do? You are reduced to the same perplexity as Dr. Johnson was, in his first trial of Bark.

During my experiment with the very bad and long neglected cases in the Non-Military General Hospital, this was the bane of all my pleasure ; for I would really have enjoyed my practice there, but for the anxiety these vomiting patients sometimes caused me. When I went into the ward, I would ask how Smith or Jones were ; the Apothecary would tell me he was very bad, and had brought up all his medicine. There was no time for delay ; the patient, if he could not retain his medicine, would probably die in the night, and I would go to his bed side, and persuade him perhaps with difficulty to swallow another dose, and before I left the ward, to my disgust, I heard the sound of vomiting again, till sometimes I was much perplexed. However, by not allowing the patient to drink anything, and by giving the Quinine in pills, Port Wine, Soup, or if he were a Spirit-drinker, in Brandy, I always contrived to give him sufficient to check the Fever, and then his stomach would bear more. But still it was only by very careful management that I did so, and always with great discomfort and distress to the patient. In the General Hospital I had no choice, I was obliged there to continue the simple Quinine, or give up my experiment ; but since then, while I was in Burmah with my Regiment, and was trying Calomel and Quinine in large doses, for the treatment of malarious dysentery, I soon became convinced, how much one drug assists the other in producing its peculiar constitutional effects. There are many cases of Fever which I described above, which cannot be salivated with the largest doses of Calomel, aided by Mercurial frictions ; but if you add Quinine to these doses, you will find that salivation quickly appears. There are also cases where it is difficult to cinchonise, which if you administer Calomel, you will find easily effected. Dr. Morehead notices the same fact, page 205. He says : " I can state from my own observation, that it is by no means an unusual occurrence, " in the course of Remittent Fevers, which in their early " stages have required the exhibition of several full doses of " Calomel, to observe, after the recurrence of the Fever has " been prevented by Quinine, a slight mercurial action to " appear on the second and third day, though not more than " a few grains of Calomel or Blue Pill, in combination with " Quinine, had been given during those days." Dr. Watson also, and other writers on the treatment of Intermittent Fever,

notice how much better and more quickly Quinine acts the next day, if a full dose of Calomel be given over-night.

Again, when I left my Hospital, and had to treat children for Fever, I found the little things sometimes obstinately refuse to take large doses of the bitter medicine, and often, if they did take it, it was vomited. And I confess that I rarely now treat a young child for *bad* Fever without mixing Calomel with the Quinine. Its results with them, without the necessity of producing any visible constitutional effects, are marvellous; and the quantity of Quinine you are obliged to give them, is greatly reduced. My rule, therefore, now is, if a patient can take Quinine in the quantities I think necessary for his safety, I give it only: but if he vomits often, I give him a full dose or two of Calomel, and I prefer the old scruple dose to any other, from its inimitable effects in checking vomiting. I never have had occasion to exceed two doses, and I find I can then continue my Quinine, without any trouble to myself or patient. The Quinine also need not be stopped, but given with the Calomel. The Calomel put dry on the tongue, and the Quinine given in Pills. To a child I give one large dose of Quinine to start with, and try and repeat it, but if the little thing is sick and refuses it, I drop two or three grains of Calomel dry on its tongue, and mixing three to five grains of Quinine in Jelly, make it into a pill, and put it with the end of my finger into the child's throat. This cannot be done so easily with ten grains, which without the Calomel would be required. Very soon, however, you will find that this treatment will check the Fever, and you can then stop the Calomel and continue only the Quinine.

There is no necessity to salivate any one, or give a grain of Calomel more than is just sufficient to assist the Quinine, and in many cases it will be found a great comfort both to the patient and physician to give it in this way.

I believe myself, that as Quinine is an antidote, so is Mercury, and that the two in combination have a more powerful and immediate effect than separately. Were we to be so unfortunate as to lose Cinchona, which to us, residents in the Tropics, would be as sad a loss as the fire which Prometheus restored, we must resort to the Calomel system again, for bleeding alone and purgatives will not check the paroxysms of Remittent Fever. It requires, as Dr. Johnson soon found, something else to propel the stagnant congestions, and the

salivation system only began to yield, as Quinine was gradually introduced. In the chronic forms of malarious disease, where we have time to produce the constitutional effects of Arsenic by small doses, we could use it, for the results of Arsenic in Ague are far more permanent than Quinine.

In conclusion, I must, in self-defence, explain other details in my practice. My published treatment may induce my readers to suppose that I treat every case of Fever with half drachm and scruple doses of Quinine, and push it to the utmost. But it must be recollected that my published cases, and the treatment I have recommended, are applicable only to extreme cases, where there is urgent danger for life. Also I must be pardoned, in performing an experiment on Quinine in the General Hospital, for pushing my treatment to extremes. Half the value of the results of my experiment would have been lost if I had not done so, for it is only the extremes of any practice which show its really hurtful tendencies. And I confess that I might have saved much of the Quinine which I gave to my patients during my trial. But I might have lost some of them, had I adopted this economy. I never witnessed any harm from over-doses, and I thought it right to settle finally the disputes about the danger of Quinine in head symptoms and local congestions, to push it to extremes: and no one, I think, can be sorry that I did so, not even my patients. But I do not do so now, I frequently only give 10 grain doses of Quinine to Fever cases, oftener, still, only 5 grains three times a day, and to a few none at all, but treat them with a purgative or two, low diet, and rest in bed. In justice, however, to your patients, you ought to give them a little Quinine, for why let them be sick when they may be well so much sooner. I do not, moreover, push Quinine through the paroxysm, if I have time and can safely avoid it, and prefer the sweating stage to any other for my largest dose. For the febrile disturbance of the accession is adverse to the absorption of all medicines, Quinine, Calomel, and purgatives included. All I say is, if there is danger, do so. Only a few cases also require heroic doses, but these being more particularly described, assume the most prominent position in every description of systems of treatment, and are too apt to be considered, as resorted to in every case. But when I see a patient with dull eye, cold clammy skin, tremulous tongue and oppressed pulse, or another with acute

head symptoms, delirium and impending coma, or a child with spasmodic contraction of the thumbs, squinting, or actual convulsions in a paroxysm, I do not lose time, by preparing such cases for Quinine with purgatives. I know the danger impending, and I give them (at least the adults) half a drachm of Quinine at once, and follow it up with scruple doses every three or four hours, according to the symptoms it produces, till they are fairly cinchonised; also with respect to diet, I do not offer or prescribe food for a young Soldier just attacked with Fever. I would give him little or nothing for the first day or two; but if I have a patient sent me who has had Fever for a week or more, I feel sure that he is starving, although he may have no appetite; and I persistently make him swallow small quantities of soup, and arrow-root and milk, and if I know him to have been a spirit-drinker, or even a free liver, I give him wine or spirit.

This is all which I wish to impress on my readers, that Malarious Fever is not an inflammatory disease, but the direct contrary, and requires no depletion by bleeding and drastic purgatives, and that the congestions of the liver, &c., which appear in Fever, and the morbid secretions thence arising are best cured by large and early doses of Quinine, under the use of which the congestions disperse, and with them the morbid secretions. Also that we can support the strength of the patient without danger by proper food and nourishment. Let a patient be, therefore, the first day or two of his Fever with but little food, but afterwards give it to him in moderation, and especially do not starve children.

A conversation I have heard as having occurred at a regimental mess table, has often struck me. One of the young Officers remarked in a sneering tone—Why Dr. M—— is actually bleeding his patients for Cholera! His friend who was at the table quietly remarked, do you think M—— a fool or a knave? The answer was necessarily—“Oh, neither.” Then, said his friend, you may rely upon it that M—— has very good reasons for acting as he does.

And so, in reviewing the history of Fever, reason, and good reason, appears clearly through all the extraordinary vicissitudes of its treatment. For Lind and Hunter are proved undoubtedly right, Johnson, too, was right, and if we had no Quinine, we must return to his practice, and if we adopted the cure of Fever by salivation, we must also deplete freely,

for otherwise, without blood-letting it is impossible in bad Fever to procure the absorption of the Mercury and salivation in sufficient time to save the patient.

Twining also, finding depletion the fashion and the received, or, as it then was thought, necessary practice, undoubtedly performed a great service to his profession by fixing and defining clearly the exact moment when bleeding can be performed with most benefit and safety. Twining's idea of applying Mackintosh's discovery of the singular efficacy, that bleeding in the cold stage of ague has in breaking the succeeding paroxysm to Remittent Fever; and his attempt to break the accessions of the acute, as Mackintosh did of the chronic form, was, to say the least of it, brilliant; and any one who reads his book can see talent sparkling in every page. In fact, this improvement of Twining is visible in every succeeding writer, and forms the basis of the treatment of our most modern authority, Dr. Martin; and the same rule is applicable to purgatives, leeches, and emetics; they ought to be given so as to act just before the paroxysm *commences*, never so as to act at its close, or the commencement of the sweating stage, for they may then, during nature's exhaustion, be fatal.

I reached India soon after Dr. Twining's death, when the treatment of Tropical Fevers was at its lowest ebb, for Medical Officers were distrustful of all treatments, and used no fixed principles. As an instance of this distrust of all medicines during this era of inertia, I will quote the following from Dr. Morehead, a celebrated practitioner during this period, page 189, vol. 1. "When Remittent Fevers have passed into the almost continued form, they are liable to evince a train of adynamic phenomena, and then the only mode of managing them is to recollect the principles laid down by Cullen 'that Fevers tend to cure themselves,' and this was a doctrine openly avowed by many of my contemporaries, but it is so no longer, for, in the words of Dr. Ewart, our Historian, 'The treatment of Malarious Fevers has now arrived at a pitch of perfection and certainty, which, judging from the records of the past, could never have been paralleled. We have now to guard against retrogression into less successful methods, and the fascination of innovations.'"

This is undoubtedly true, for, with Calomel to assist, I scarcely know one inconvenience attending our present practice, and ~~to~~ its

efficacy is as certain as correcting an acid by an alkali. We hold in our hands positive and certain antidotes, which we have only to use with common discretion, and I boldly assert that no uncomplicated case of Malarious Fever ought ever to be lost, if we see it in reasonable time, and the cure now follows so certainly, and with so little suffering to the patient, that it is a positive and noble pleasure to treat cases of Remittent Fever. I have done my share towards this great end, but I claim no credit for talent or originality. I have simply put together in their right places the materials of others, and the real secret of my success has been an unwavering and determined belief in the testimony of others, and particularly of Army Surgeons as Lind and Hunter, who had no private interests to serve.

I believe myself that valuable truth has been the basis of all our great changes in medicine, and we would be much more usefully employed in searching for this truth, and trying to separate it from its encumbrances of false theory, than in sneering and pushing aside the whole as absurd in our modern self-sufficiency.

ABSCESS OF THE LIVER.

During my service with my Regiment in the Burmese War, and afterwards in the siege of Delhi, Abscess of the Liver was very frequent, and I had numerous cases by which to test the efficacy of the usual methods of treatment.

Annesley recommends, vol. 1, p. 655 "that all Abscesses of the Liver should be opened with a common Abscess Lancet. He says, being satisfied that the Abscess has sufficiently advanced, and has adhered to the external wall of the abdomen having made a large external incision, an Abscess Lancet should be introduced, and the tumour laid open to the full extent of the external wound, which ought to be from $2\frac{1}{2}$ to 3 inches in length. The purulent collection having been fully evacuated, the cavity should be filled with lint, which gives a mechanical support to the excavated parts, and the wound dressed with compresses and bandages in the usual way." One of his cases, 124, page 658, is remarkable. He says "in opening the Abscess, the wound was made about 3 inches in extent, and on introducing my finger, I found the abscess considerably higher than where we had made the opening, although this appeared to be the

“most depending part of the tumour. From the wound
 “being large, we were enabled to pass two or three fingers,
 “so as to support the tumour, and with the point of a seal-
 “pel we opened it, making our hand the medium of con-
 “ducing the pus, for we found the Abscess pendulous and
 “the adhesions doubtful; 16 ounces of eurdled pus was thus
 “discharged.”

This is one mode of opening an Abscess of the Liver, and from the case quoted, it will appear how difficult it is, even for practitioners like Dr. Annesley, to decide when adhesions have formed, without which this operation is extremely dangerous; for some of the matter almost necessarily must escape through the large opening between the Liver and the Ribs into the cavity of the Abdomen.

The other method of treating the Abscess, which this operation was intended by Dr. Annesley to supereede, was to puncture the Abscess with a Trochar, but the pus, he says, “was often so thickened, that it would not pass through it.”

Dr. Budd writes, 3rd edition, page 123—“In opening an
 “Abscess of the Liver, there is another and more unavoidable
 “source of danger, which has not been noticed by the
 “writers to whom I have referred. It is that the solid
 “Hepatic Tissue cannot readily collapse so as to close the
 “cavity when the Abscess is opened. When, then, a free
 “opening is made, even into a recent Abscess, air almost
 “necessarily enters the cavity, and from the sudden removal
 “of pressure, or it may be from the manipulation employed
 “to empty the cavity, violence is done to the walls of the
 “Abscess, and there is often some degree of Hæmorrhage
 “from them. Air and blood thus become mixed with the
 “pus in the Abscess, decomposition takes place, and the air
 “or decomposed pus sets up fresh inflammation of the inner
 “surface of the sac. This causes, of course, a fresh accession
 “of Fever, and constitutional disturbance; and if the Abscess
 “be large, a profuse fetid and continuous discharge, which
 “may soon exhaust the strength of the patient. The second-
 “ary inflammation thus excited, by the presence of air or the
 “decomposed pus, may even lead to Gangrene.”

“In consequence of this secondary inflammation, it is best
 “when the Abscess of the Liver projects at the side, to allow
 “it to open of itself. When the Abscess opens of itself, it
 “is usually by a very small aperture, like those in worm-eaten

“wood, which never closes; and the matter gradually oozes out as the sac contracts. No air gets mixed up with the matter of the Abscess, no violence is done to its walls, and no fresh inflammation is set up.”

The disadvantage of Dr. Budd's method, is the number who die from the irritation of the Abscess, before this slow process of nature is accomplished; and the large majority of cases where this pointing and opening never takes place, although the position of the Abscess can be easily recognised externally. In such cases the pus generally finds its way into the peritoneal cavity, where it necessarily causes death. The majority, however, of these cases can be relieved by the operation which I propose.

The failure of Annesley's operation was the large opening required, causing escape of pus into the Abdomen, frequent Hæmorrhage from the Liver, and extensive injury of its substance; also the extreme irritation caused by stuffing the Liver with lint.

In the first case of Liver Abscess which I treated, which seemed suited for external opening, I asked for a consultation of the Superintending Surgeon and others, and it was decided that the Abscess was to be opened by puncture with a Trochar. The Abscess was very large, and the pus fortunately liquid, and $3\frac{1}{2}$ pints were extracted. It was then decided to remove the Canula to avoid the passage of air, which had been further secured by drawing up the skin before puncturing. So that when the Canula was removed, the skin taking its natural position, acted as a valve to close the internal opening. The patient improved wonderfully, and showed evident symptoms of recovery, but the Abscess began to fill again, and after a week was as large as before, and he began to sink again under hectic and the irritation. I then punctured the Abscess again, but this time kept the Canula carefully in the puncture, retaining it there by straps of adhesive plaster. I had previously injected warm water by means of the Indian Rubber bag, used as a Hydrocele Syringe, repeatedly into the Abscess, through the Canula, allowing it to run out, and injecting again, till the Abscess was thoroughly cleansed of all remaining pus, and the water returned clean. This was done to prevent the decomposition which always takes place, in the pus remaining in the Abscess, after it is opened and exposed to the air. This process of washing

out the Abscess was repeated twice a day, so that no decomposition could take place in the pus as it re-formed. The operation never caused any inconvenience to the patient, and he merely required to be placed on his right side with pillows to allow the water after injecting to return through the Canula. Occasionally if the Canula became obstructed, a probe was gently passed to clear it. Liberal diet was allowed, and under this treatment the patient rapidly recovered without one bad symptom. The Abscess was carefully washed free from pus twice a day, the discharge became daily less, and when it ceased, the Canula was removed, and he eventually quite recovered.

After this favourable result, I tried the same method with all my cases, and they were at that time very frequent, and I am satisfied that the results obtained by it were far better than by any of our hitherto published treatments.

I found, too, that the pus could always be made sufficiently fluid by the injected water, to pass through the Canula. Also by this method, it is of no consequence whether adhesion between the costal surface and the Liver has taken place in front of the Abscess, because the Canula is embraced closely by the peritoneal covering of the Liver, and no matter can escape by its sides. The danger, moreover, of Hæmorrhage, which is constant, when a 3-inch incision is made into the Liver, is avoided by this simple puncture.

It was often astonishing, how rapidly the patients rallied from apparently the last extremity. I never saw any bad result from the operation itself; the chief cause of death after it, was the very frequent occurrence of other Abscesses existing in the Liver at the same time. My experience also taught me to place more reliance on the assertions of some Surgeons in Bombay, that the Liver may be much more freely punctured than is generally supposed, and in any case in which I saw danger to life, from the irritation of an Abscess in the Liver, and could detect the slightest bulging, I would without any hesitation give the patient a chance of life, by puncturing the swelling with a Trochar.

I have in some cases missed the Abscess, but the pus afterwards found its way through the puncture. Again, in other cases, the patient has died from the unopened Abscess, and I have examined the course of my puncture and found no injury whatever, but a thin line of lymph in its course. On

the whole, I feel certain that a very large proportion of cases of Abscess of the Liver, who now die by too long neglect of the operation, might be saved thus by a little boldness. An Officer of my Regiment was shot with a musket ball through the substance of the Liver, from the right side on a level with the Gall bladder to the spine, from the skin covering which, I extracted the ball, and yet he recovered and enjoyed good health, though the injury to the Liver was so great that the whole right side contracted, as was also the case with the patient from whom the $3\frac{1}{2}$ pints of pus were extracted.

I may remark, finally, that of the large number of patients with Abscess of the Liver which I have witnessed, the majority have resulted from ulceration of the colon in Dysentery; and, in these, the Abscess is rarely single; mostly there are three or four Abscesses of various sizes. Some few very large Abscesses have appeared with almost no previous symptoms, and these are always single, and a few solitary Abscesses have resulted from falls or other direct injuries of the Liver, but I have never yet seen an Abscess result from Remittent Fever.

TABLE No. 1.

Cases of Fever treated in the Military Hospital, Calcutta, during the year, November 1st, 1849, to November 1st, 1850, of Her Majesty's 70th Regiment.

	Treated by the Surgeon of H. M.'s 70th Regiment.	Treated by Assistant Surgeon E. Hare.
Remained	13	
Admitted	266	292
Discharged	267	281
Transferred	2	4
Died	4	2
Remaining	6	5
Total	279	292
Deaths, 1 in $69\frac{3}{4}$ treated.		Death, 1 in 146 treated.

J. FORSYTH, *Senior Surgeon,*
Secretary, Medical Board.

From Medical Board's Report of 1849-50.

TABLE No. 2.

Cases of Fever treated in the General Hospital (Non-Military) Calcutta, from 1st November 1849 to 31st October 1850.

	Treated by Assistant Surgeon E. Hare.
Remained
Admitted	129
Discharged	120
Transferred	3
Died	1
Remaining	5
Total	129

Death, 1 in 129 treated.

J. FORSYTH, *Senior Surgeon,*
Secretary, Medical Board.

From Medical Board's Report of 1849-50.

TABLE No. 3.
General Hospital, Calcutta.

YEAR.	FEVER.		DYSENTERY.	
	Admis- sions.	Deaths.	Admis- sions.	Deaths.
1830	639	45	144	22
1831	406	38	128	20
1832	409	38	124	20
1833	856	107	128	19
1834	605	77	147	39
1835	233	16	71	20
1836	113	12	55	10
1837	99	4	64	15
1838	112	18	52	13
1839	194	13	78	15
1840	221	21	68	11
1841	289	33	172	42
1842	414	29	147	41
1843	209	10	88	19
1844	235	16	141	27
1845	208	15	91	31
1846	183	21	87	27
1847	204	17	79	17
1848	243	18	88	25
1849	488	17	94	28
Total	6,360	565	2,046	461
Fevers, 1 death in 11·256 admissions.			Dysentery, 1 death in 4·438 admissions.	

J. FORSYTH, *Senior Surgeon,*
Secretary, Medical Board.

From Medical Board's Report, 1849-50.

TABLE No. 4.
Military Hospital, Calcutta.

YEAR.	FEVER.	
	Admitted.	Died.
1830.. .. .	522	38
1831	353	15
1832..	255	9
1833	538	15
1834.. .. .	503	18
1835	317	8
1836... .. .	288	12
1837	246	7
1838... .. .	107	4
1839	361	1
1840... .. .	537	16
1841	542	13
1842... .. .	840	19
1843	783	39
1844..	182	1
1845	158	2
1846... .. .	179	6
1847 to April 1st	36	1
1847-48	268	6
1848-49... .. .	512	18
1849 to December 1st	550	22
Total	8,080	270

Ratio of Mortality.

From 1830 to 1849, 1 death in $29\frac{3}{4}$ admissions.

J. FORSYTH, *Senior Surgeon.*

Secretary, Medical Board.

From Medical Board's Report, 1849-50.

TABLE No. 5.

Return of Fever Cases treated in the 1st and 2nd European Bengal Fusiliers from 1st April 1852 to 1st January 1861, taken from the Annual Returns in the Office of the Principal Inspector General, Calcutta.

YEAR.	FEVER.		
	Admitted.	Died.	
1852	1,264	1	
1853	810	7	
1854	362	1	
1855	445	4	
1857	1,644	14	
1858	1,859	3	
1859	337	0	
1860	261	3	
Total... ..	6,982	33	

1 death in 211·19 admissions.

TABLE No. 6.

Exhibiting the annual quantity of Quinine and Cinchona Bark issued from the Honorable Company's Dispensary at Calcutta, during the ten years from 1849-50 to 1858-59 inclusive.

YEAR.	QUANTITY OF QUININE.			QUANTITY OF CINCHONA BARK.		
	lbs.	oz.	dm.	lbs.	oz.	dm.
1849-50	520	7	0	2,543	9	0
1850-51	513	9	0	2,572	12	0
1851-52	628	11	0	4,695	2	0
1852-53	811	3	0	3,371	4	0
1853-54	637	15	0	2,129	10	0
1854-55	773	0	0	3,881	1	0
1855-56	985	8	14	3,258	11	0
1856-57	1,050	5	14	4,922	4	0
1857-58	1,656	9	4	5,159	15	0
1858-59	1,521	5	6	3,567	9	0

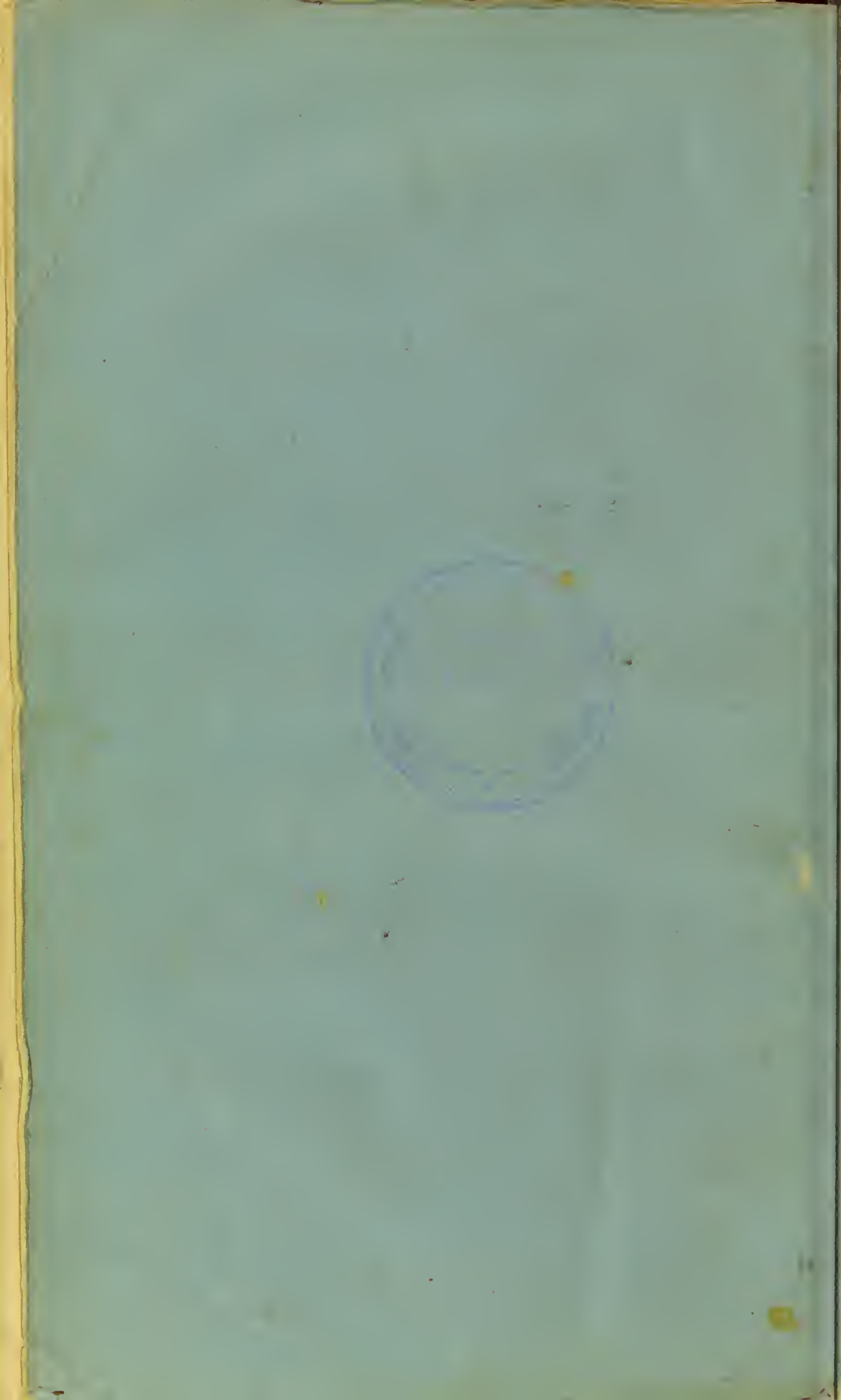
(Signed) ALEXANDER GRANT,

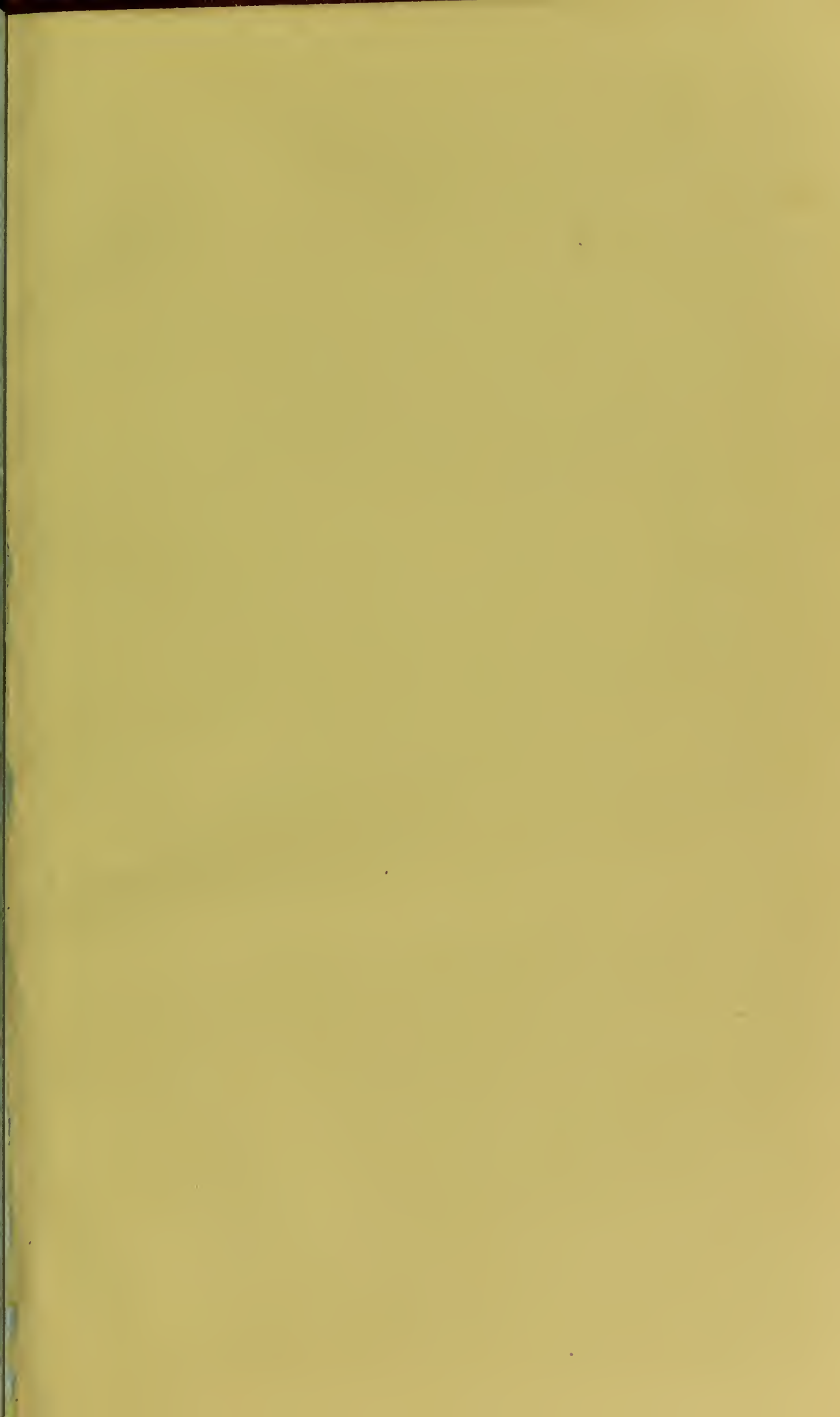
Apothecary General, Calcutta,

From Medical Annals, No. 13, page 380 of 1861.













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